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AND

JOURNAL OF SCIENCE.

CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S.L. & E. &c.
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"Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

—"Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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ERRATA.

Page 248, line 8, *for* heat *read* cloud.
— 252, fig. 2, the line O P, and not I P, should have been produced.
— 362, last line in Table II., *for* 15·218 *read* 15·18.
— 363, „ III., *for* pressed 1·668 *read* hard-drawn 0·1399.
— 365, middle of page, *for* 0·9750 *read* 0·9742.
— 366, „ *for* 0·9632 *read* 0·9625.
— 366, bottom of page, *for* 0·9723 *read* 0·9716.
for 0·9605 *read* 0·9598.
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THE

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PHILOSOPHICAL MAGAZINE

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JOURNAL OF SCIENCE.

[FOURTH SERIES.]

JANUARY 1865.

I. *Researches on the Mineralogy of South America.* By DAVID FORBES, *F.R.S.*, *&c.**

I.

BISMUTH.—This metal occurs in the native state, as well as in combination with oxygen, sulphur, and tellurium, in a small vein in the lower Silurian clay-slates of the mountain Illampu† in Bolivia. The mine of San Baldomero opened upon this vein is situated but little under the line of perpetual snow, and has an elevation of between 14,000 and 15,000 feet above the level of the sea. The vein cuts through the lower Silurian strata, here tilted up at a very high angle (nearly vertical), and has to some extent altered these strata: in immediate contact with the vein, a sahlband of from a few lines to some inches in width is seen in the rock on each side of the vein, the thickness of the sahlband being dependent on the relative strength of the vein itself; this is much darker in colour than the less altered rock, and has evidently been altered in chemical composition by the action of the mineral matter of the vein, most probably by the sulphur and arsenic present; at a greater distance the slates are only hardened or, as it were, baked by heat, and not more altered than that the abundant fossils in them are well preserved —as various species of *Homalonotus*, *Orthis*, *Arca*, *Tentaculites*,

* Communicated by the Author.

† This mountain is in England generally, but erroneously, called Sorata, owing to its being frequently called by the Spaniards "La nevada de Sorata" from the town of that name situated at its base. Its altitude has been determined to be 24,812 English feet above the level of the Pacific Ocean; and consequently it is the highest of all the peaks of the Andes of South America.

Ctenodonta, &c.; in fact hand specimens were obtained consisting of ore and veinstone containing, in the latter, fossils in a perfectly recognizable condition.

The geological examination of the environs appeared to prove that this and other veins in the neighbourhood had been produced by the eruption of the large mass of granite (auriferous) further to the west, with which the veins are most probably contemporaneous.

The age of the granite itself was considered to be of the middle Silurian period.

The minerals found present in this vein were—Danaite or cobaltiferous mispickel, mispickel, bismuth, bismuth-glance, bismuth-ochre, carbonate of bismuth, telluric bismuth, gold, apatite, iron pyrites, zincblende, tourmaline, epidote, calcspar, and quartz. The main mass of the lode was Danaite, which also formed the base for the mining exploration.

The native bismuth was found more abundantly on the surface of the vein, and became very rare as the workings in the mine increased in depth; it occurred in irregular lumps or masses, and frequently as if wedged in between the walls of the lode. The largest mass found in one piece weighed about 400 pounds, and had one side entirely overlaid with a thin plate of gold of a rather light colour, not more than 0·05 inch in thickness, adherent and as if soldered to the bismuth.

No crystals of bismuth were observed; but on fracture the basal cleavage was perfect, and the fresh surfaces showed the peculiar reddish-white colour so characteristic of this metal.

The specific gravities of two separate specimens quite free from extraneous matter were found respectively to be 9·98 and 9·77 at 60° Fahr. The analysis was conducted as follows:—

The arsenic was determined by fusing 25·08 grs. in fine powder, with a mixture of 80 grs. pure nitrate of potash along with 100 grs. pure carbonate of soda in a silver crucible; the mass was extracted with water, acidified by hydrochloric acid, and the arsenic precipitated as arseniate of magnesia and ammonia by the addition of a mixture of ammonia, chloride of ammonium, and sulphate of magnesia. 0·270 gr. arseniate of ammonia and magnesia were obtained, equivalent to 0·097 arsenic, or to 0·38 per cent. arsenic in the bismuth.

The sulphur was determined by suspending 50·01 grs. bismuth in finest powder in a solution of potash, and passing a stream of chlorine gas through until all the bismuth was oxidized (this required a very long time); the solution was then rendered more acid by addition of hydrochloric acid to prevent any tellurate of barytes being simultaneously precipitated, and the sulphuric acid precipitated by the addition of chloride of barium.

The precipitated sulphate of baryta weighed 0·26 gr., equivalent to 0·07 per cent. sulphur in the bismuth.

The tellurium present was determined by dissolving a fresh portion of bismuth, weighing 45·55 grs., in nitric acid, neutralizing with ammonia, then adding sulphide of ammonium in excess, with which it was allowed to stand, being shaken occasionally, for some five days. The insoluble sulphide of bismuth was filtered off and washed with water containing sulphide of ammonium. The filtrate was now treated with an excess of hydrochloric acid, throwing down the sulphide of tellurium along with free sulphur and a little sulphide of arsenic; these were digested with nitric acid until the sulphur was quite pure, filtered, and the solution, previously strongly acidified and heated with hydrochloric acid to reduce all telluric acid present, was treated with a stream of sulphurous acid passed through it until all tellurium present had been thrown down, allowed to stand in an atmosphere of sulphurous acid, and then the precipitate was washed with sulphurous acid and dried with proper precautions. The tellurium thus obtained weighed 2·425 grs., equivalent to 5·09 per cent. in the metallic bismuth.

In order to see if any silver was present in the mineral, three several portions, respectively weighing 40·50, 50·0, and 92·0 grs., were cupelled; in no case was any visible trace of silver found, but a faint trace of gold was distinctly observed.

According to the above examination, the analysis of the native bismuth will now stand as follows:—

Bismuth	94·46
Tellurium	5·09
Gold	trace
Arsenic	0·38
Sulphur	0·07
	100·00

From observations on the specific gravity of other specimens of bismuth from this mine, I think it most probable that the percentage of tellurium may vary greatly in different specimens, which is confirmed by the analysis of another specimen from the same mine*, sent some years back to the United States, and which was a portion of the largest piece before mentioned. The analysis is by Mr. F. A Genth, and the results as follows:—

Bismuth	99·914
Iron	trace
Tellurium	0·042
	99·956

* American Journal of Science and Arts, 2nd series, vol. xxvii. p. 247.

Bismuthine.—As before mentioned, this mineral is found at the San Baldomero mine, where it occurs both in the form of scales incrusting the masses of native metallic bismuth, and by itself in small compact masses having a very highly developed foliated structure. It occasionally occurs in long and fine acicular crystals, which sometimes have an iridescent lustre, and frequently are so intermingled with slender needles of black tourmaline as to present a very peculiar and at first glance puzzling appearance.

Frequently perfect single crystals of mispickel are found imbedded in the more compact variety. The largest specimens found never exceeded half a pound in weight; in no case were defined crystals obtained, and in only one case were the needles or plates observed to have terminal faces.

Two perfect cleavages, with occasionally a less distinct third cleavage-plane, were present, and the faces of cleavage invariably possessed a brilliant metallic lustre.

Hardness was never found above 2, and sometimes appeared rather less.

Specific gravity of a pure very compact specimen was found to be 7·16 at 60° Fahr.

The chemical examination was conducted as follows:—The determination of the sulphur was made by heating 20·02 grs. of the pure mineral in finest powder, with a mixture of 60 grs. pure nitrate of potash along with 70 grs. pure carbonate of soda in a silver crucible; it glowed gently and became semifused, but did not flow. The mass, on cooling, was digested with water and thrown on to a filter to separate the oxide of bismuth, which was well washed. The filtrate was then acidified with hydrochloric acid and precipitated by the addition of a solution of chloride of barium. The sulphate of barytes thus thrown down was collected as usual, and weighed 28·46 grs., equal to 3·926 grs. sulphur, or 19·61 per cent. sulphur in the mineral. The bismuth was now estimated by dissolving the oxide left upon the filter, as above mentioned, in nitric acid, and precipitating the solution with carbonate of ammonia in excess. The precipitate, well washed and ignited, afforded 18·03 grs. oxide of bismuth, corresponding to 16·202 grs. metallic bismuth, or 80·93 per cent. in the mineral.

An equal amount of the original mineral was examined for arsenic, using the process employed in the case of examining native bismuth for this substance; no trace however was found*.

* I may mention that in analyzing another specimen of this mineral I found 1·90 per cent. arsenic present; but I satisfied myself that this was due to fine grains of mispickel interposed between the laminæ of the mineral.

Silver, iron, nickel, and cobalt were also specially examined for, but not found present in the mineral.

42·01 grs. of the mineral in fine powder were now examined for tellurium by the same process as is described in the case of native bismuth. No tellurium, however, was found present. The analysis will therefore stand as follows:—

Bismuth . . .	16·20	or	80·93 per cent.
Sulphur . . .	3·92	„	19·61 „
	20·12		100·54

Considering the atomic equivalent of bismuth as 106, and that of sulphur as 16, the formula $Bi^2 S^3$ would require the following percentage composition:—

Bismuth, 2 atoms . . .	212	or percentage	81·53
Sulphur, 3 atoms . . .	48	„	18·47
	260		100·00

Mispickel.—This mineral only occurs crystallized in the San Baldomero mine when in small drusic cavities or close to the walls of the vein, where the crystals are frequently found protruding from the surface of the massive mineral and imbedded in carbonate of lime, or, as before stated, in the bismuthine. All the crystals I have observed belong to the trimetric system, and are rhombic octahedrons more or less modified, most generally as twin crystals. Perfect octahedrons are occasionally found, but only when imbedded in the soft bismuthine. No cleavage-planes could be observed. Colour: silver-white to grey-white; but fresh surfaces tarnish quickly, becoming of a brassy colour on exposure.

The specific gravity was found to be 6·255 at 60° Fahr. When heated alone, the mineral first evolves sulphide of arsenic and then metallic arsenic, and leaves sulphide of iron containing a trace of cobalt and nickel.

The chemical analysis was conducted as follows:—

23·42 grs. in finest powder were intimately mixed with 60 grs. pure nitrate of potash previously mixed with 60 grs. pure anhydrous carbonate of soda, and fused in a silver crucible; the mass deflagrated very gently, but a strong smell of arsenic was observed; it did not flow; the whole on cooling was digested with water, and, after filtration and washing, the filtrate was acidified by hydrochloric acid, and the sulphuric acid precipitated by addition of a solution of chloride of barium. The sulphate of barytes collected amounted to 33·16 grs., equal to 4·575 grs. sulphur, or 19·534 per cent. in the mineral.

The insoluble oxides were now dissolved in hydrochloric acid, neutralized by ammonia, and the iron separated from the cobalt,

nickel, and manganese present by the use of succinate of ammonia with the usual precautions. The succinate of iron, redissolved in hydrochloric acid and reprecipitated by ammonia, afforded 11·78 grs. sesquioxide of iron, equal to 8·173 grs. iron, or 34·47 per cent. in the mineral.

The solution, or rather filtrate, separated from the oxide of iron was now precipitated by sulphide of ammonium; this precipitate, collected, incinerated, dissolved in nitric hydrochloric acid, and precipitated by potash, gave 0·19 gr. oxide of cobalt, nickel, and manganese, which were arsenized and separated according to Plattner's method by the blowpipe, giving 0·171 quadribasic arsenide of cobalt, equal to 0·104 metallic cobalt, or 0·44 per cent. in the mineral, and 0·09 quadribasic arsenide of nickel, equal to 0·0056 metallic nickel, or 0·03 per cent., whilst the manganese was estimated by the difference. The determination of arsenic was attempted in precisely the same manner as that of the sulphur, as described above; but notwithstanding every precaution, I could not have any confidence in the result, as after three attempts I noticed in each case the smell of garlic indicative of some evolution of arsenic. I therefore modified the process as follows:—20·05 grs. mineral in fine powder was placed in a porcelain crucible and drenched with pure nitric acid until no further oxidation took place. Carbonate of soda was added to take up the superfluous nitric acid, and more carbonate of soda along with nitrate of potash added to make up the same quantities as those used in determining the sulphur. The whole was heated to dryness, removed to a silver crucible and fused; the residue was digested with water as usual, and the arsenic determined in the filtrate, after acidifying it by hydrochloric acid, as arseniate of ammonia and magnesia, which, when ignited with the usual precautions, gave 18·84 grs. $2MgO\,AsO^5$, equal to 9·116 grs. arsenic, or 45·46 per cent. arsenic in the mineral*.

The results of this examination, when tabulated, will now stand as follows:—

Arsenic	45·46
Sulphur	19·53
Iron	34·47
Manganese . . .	0·14
Cobalt	0·44
Nickel	0·03
	100·07

* I had the curiosity to make a determination of the arsenic by simply fusing with nitrate of potash and carbonate of soda as before mentioned, and only obtained 38·59 per cent. of arsenic, a loss of 6·87 per cent., which shows how unsafe this method is.

The formula $FeS^2 + FeAs$ would require—

Arsenic, 1 atom . . .	75·0	or percentage	46·02
Sulphur, 2 atoms . . .	32·0	,,	19·63
Iron, 2 atoms	56·0	,,	34·35
	163·0		100·00

Danaite.—In the analysis of the mispickel above given, I had fully expected to find considerably more cobalt than actually was found; for it must be remembered that this mispickel had crystallized out of the Danaite, to which it bears a proportion quite insignificant, as the Danaite forms the entire mass of the vein, which occasionally has presented a solid mass of the mineral having a breadth of more than ten feet between the walls.

The Danaite was never found crystallized, but formed a compact mass, possessing a granular, fibrous, or foliated structure, and more or less crystalline appearance. The specific gravity of the fibrous variety, which probably is the most pure, was found to be 6·94 at 60° Fahr., of the granular variety 5·86; but this was invariably intermixed with more or less quartz or gangue in a very fine state of dissemination. Several analyses were made by the methods described under mispickel, and the following percentages show the average composition derived from their results:—

Arsenic	42·83
Sulphur	18·27
Iron	29·22
Manganese . . .	5·12
Cobalt	3·11
Nickel	0·81
Bismuth	0·64
	100·00

which corresponds to the same formula as for mispickel, allowing a portion of the iron to have been replaced by the cobalt, nickel, and manganese here present.

Nickeliferous Mispickel.—From Mr. Philip Kroeber I received a specimen of this mineral which I have not before seen described in any of the published works upon mineralogy. The mineral occurs as a vein in the lower Silurian slates in the Cordilleras between La Paz and Yungas in Bolivia, about 5 leagues from Unduavi, at a place called by the Indians Chacaltaya. The geological relations of this vein are very similar to those described in the case of the vein of San Baldomero, and it has doubtless originated from the same cause, the outburst of the same granite.

More or less distinct crystals occur, and pertain to the trimetric

system. The prisms are found striated parallel to their longer axis, and twins are most common. Lustre metallic; colour tin-white to steel-grey, tarnishing quickly to a greenish-grey tinge. The crystals, when situated in quartz, are frequently coloured a fine green on their surfaces, evidently from the nickel present. The ore also, when exposed to the air and weather, assumes a fine apple-green colour from the same cause. Streak black to dark grey. Fracture uneven, brittle. Specific gravity 4·7 at 60° Fahr. A chemical examination was made by Mr. Philip Kroeber which gave the following results:—

Arsenic	43·683
Sulphur	16·755
Iron	34·932
Nickel	4·741
Cobalt	trace
Silver	0·090
Gold	0·002
Antimony . . .	trace
	100·203

Mispickel.—Another variety of mispickel, occurring likewise in veins in the lower Silurian formation, is found at Inquisivi in Bolivia, where it is associated with Danaite, native bismuth, and an undetermined and probably new mineral in hexagonal crystals with perfect basal cleavage and having a great resemblance to dark brown zincblende, in a large quartz vein.

The crystals obtained were too imperfect for measurement, but evidently belonged to the trimetric system; no defined cleavage-plane was observed. Colour dark steel-grey; lustre dull metallic; streak dark greyish black. Non-magnetic. Hardness 5·5, scratching apatite, but not orthoclase.

The specific gravity at 60° Fahr. was found to be 6·170.

Before the blowpipe in a closed tube it gave off, first some trace of sulphur, then sulphide of arsenic, and lastly metallic arsenic.

In the oxidizing flame it gave off sulphur and arsenic; and with borax glass, slagged off entirely with reaction for iron, but showed no appreciable quantity of copper, nickel, or cobalt present.

An assay by cupellation afforded a trace of silver containing gold, which latter appeared present in larger quantity than the silver.

The analysis was performed exactly as in the case of the mispickel from San Baldomero mine, with the exception that the arsenic was determined as loss. The results were as follows:—

Arsenic	46·95
Sulphur	18·12
Iron	34·93
Cobalt, nickel, bismuth, silver, and gold	traces
	100·00

which conforms to the usual formula for mispickel, $FeS^2 + FeAs$.

I may mention that I examined the Danaite which is associated with this mineral, and found in various specimens a percentage of cobalt (with trace of nickel) which varied from 1·81 to 3·9 per cent. metallic cobalt.

Antimonial Galena.—Considerable veins of antimonial galena are worked between La Paz and Yungas on the eastern slope of the Andes, chiefly for the amount of silver contained in them. The mines are situated in veins which cut at very high angles through the lower Silurian slates, containing abundant fossils of the genus *Crusiana* and *Boliviana*, as well as others not yet determined. Their geological relations are otherwise similar to the veins hitherto treated of in this paper. A specimen from the Mina Pilar was examined by Mr. Philip Kroeber, who has communicated the results to me. The mineral which was analyzed was quite free from gangue, but contained some minute specks of iron pyrites and chalcopyrite; compact but foliated in structure, and in colour of a light steel-grey tint and very lustrous. Its analysis afforded him—

Lead	62·510
Antimony	15·379
Copper	2·461
Iron	0·853
Silver	0·190
Arsenic	trace
Sulphur	18·807
	100·200

The associated minerals are quartz, calcspar, carbonate of iron, iron pyrites, chalcopyrite, zincblende, cubic galena, Fahlerz, and a new mineral, in strongly magnetic, copper-coloured crystals, which I have called Kroeberite, and which, though not yet analyzed, appears to be principally a subsulphide of iron. In some specimens also there occurs some mineral containing cobalt, most probably "Selenkobaltblei," as the ore was found to contain on analysis more than 2 per cent. of cobalt.

If we look upon the iron and copper as combined with a portion of the sulphur, forming iron or copper pyrites, we may suppose the mineral here analyzed to pertain to the mineral species Geokronite. I add a tabulated statement of the various

minerals considered as compounds of the sulphides of antimony and lead, for the sake of comparison.

Specific gravity.	Name.	Formula.	Sulphur.	Antimony.	Lead.
5·3	Zinckenite.	$PbS + Sb^2S^3$	21·6	43·5	39·4
5·5	Jamesonite.	$3PbS + 2Sb^2S^3$	20·2	36·2	43·6
5·4	Plagionite.	$4PbS + 3Sb^2S^3$	20·6	38·3	41·1
5·6	Heteromorphite.	$2PbS + Sb^2S^3$	19·2	31·0	49·8
5·8	Boulangerite.	$3PbS + Sb^2S^3$	17·9	24·1	58·0
?	Meneghinite.	$4PbS + Sb^2S^3$	16·7	19·6	63·7
6·40	Geokronite.	$5PbS + Sb^2S^3$	16·5	16·7	66·8
6·47	Do. (Fahlun).	$6PbS + Sb^2S^3$	14·5	14·6	70·9

Veins of these antimonial galenas are met with almost everywhere, cutting through the Silurian strata of South America. They are generally crystallized, or rather more or less crystalline; and although they can be procured free from apparent mechanical admixture of other minerals, yet it is extremely difficult to determine the mineral species to which they belong merely by an examination of their external and physical characters, and it is most probable that a closer examination will show the presence of at least more than one new mineral species pertaining to this class.

The specific gravities of these varieties differ considerably, yet not sufficiently to be a decisive criterion as to the species; eight varieties of what appeared to be antimonial galena from different mines in the department of La Paz, Bolivia, gave respectively the following specific gravities taken at 60° Fahr.:—

5·25, 5·302, 5·49, 5·49, 6·515, 7·07, 7·20, 7·41;

and I have no doubt that on further trial many intermediate numbers would be found; indeed it would seem as if the sulphides of antimony and lead would combine in any proportion.

II. *On the relative Heating, by Solar Radiation, of the Soil and of the Air, on a Mountain and in a Plain. By* M. Charles Martins*.

THE sun's rays falling on the top of a mountain ought, theoretically, to be warmer than those which, after traversing the lower and denser strata of our atmosphere, reach the adjacent plain, since these strata necessarily absorb a considerable quantity of the heat of such rays. Observation fully confirms the prediction of theory in this case. All travellers who have ascended high mountains have been surprised at the great

* From the *Comptes Rendus de l'Acad. des Sciences*, October 17, 1864.

heat of the sun's rays and of the ground, when compared with the temperature of the air in the shade, or with that of the soil at night. As early as 1842, MM. Peltier and Bravais made, from the 10th to the 18th of August, a series of bi-hourly observations on the temperatures of the air and of the soil on the summit of the Faulhorn, a Swiss mountain which rises to a height of 2680 metres above the level of the sea. Experiments of a similar kind were made by Bravais and myself, two years afterwards, on the same mountain, between the 21st of September and the 2nd of October. The 125 observations comprised in these two series, and which were continued from six in the morning to six at night, both in fair weather and in foul, under cloudy as well as cloudless skies, gave, notwithstanding, a mean temperature of 11°·75 for the soil during the day, that of the air being only 5°·40. It thus became evident that during the day the soil was heated by the sun twice as much as was the air. We did not know, however, what, during the same period, had been the relative heating of the soil and the air in the Swiss plains. For a long time I was anxious to remove this incompleteness, by determining the relative heating of the same kind of soil, at the same moment, on a lofty mountain and on an open plain, when the sky was pure and the air calm. Bagnères-de-Bigorre and the Pic du Midi appeared to me to possess all the conditions desirable for experiments of this nature. The horizontal distance between the two places, as measured on the new map of the Etat-Major, is not more than 14,450 metres, and the two points are under the same meridian. The Pic du Midi, perfectly isolated from the principal chain of the Pyrenees, rises to a height of 2877 metres above the sea—an altitude which merits all confidence, since the Pic du Midi was selected as one of the principal points of the triangulation upon which the new map of France is based. I was moreover able to connect, by a single sight through a level, my point of observation in the garden of my friend Dr. Costallat, at Bagnères, with the system of levels connected with the railways of France. The point in question was thus ascertained to be 551 metres above the level of the ocean, giving a difference of 2326 metres between the altitudes of my two stations. Besides this, the valley of Bagnères has the advantage of not being one of those narrow ones where the reflexion of the sun's rays increases the temperature; its breadth, in fact, taken from crest to crest of the hills by which it is bounded on the east and west, amounts to 2800 metres. It will be admitted, therefore, that it would be difficult to find in the Alps or in the Pyrenees two stations more favourably situated for the success of the comparative observations which I had in view. These observations, however, would not have been at

all comparable with each other had one thermometer been placed at the surface of the natural soil of the mountain, whilst the other reposed on the soil of M. Costallat's garden; for, as I have elsewhere shown*, superficial soils of different natures are very unequally heated under the influence of solar radiation. In order that the experiments might admit of comparison, it was necessary to observe the heating effect on the same soil at both stations. For this purpose I chose the soil resulting from the decomposition of the wood found in old hollow willow trees; it is a vegetable soil, since plants, such as briars, honeysuckles, elders, &c., grow thereon very vigorously; it is moreover homogeneous, comparable with like soils, and easy to procure in all countries. Its absorbing power is a mean between those of seven other kinds of soil with which I have compared it; its conducting-power for heat is inferior, however, to that of any of the seven, whilst its emissive power is greater.

The experiments were made in the following manner:—A wooden box, whose length and breadth were equal to 20 centimetres, and whose depth was 10 centimetres, was filled with the above soil, and a thermometer with a cylindrical bulb was laid upon its surface—the bulb and the tube, up to zero, being covered with a thin layer of soil. Another thermometer, which had a bent tube, was placed so that the bulb was 5 centimetres below the surface of the willow soil. This box was placed on an isolated hillock in M. Costallat's garden, and another exactly similar one was placed on the summit of the Pic du Midi by my assistant, M. Pierre Roudier, who for twelve years has assisted me in my meteorological researches with great zeal and intelligence. Both boxes were buried so that their surfaces were on a level with the natural soils at the two stations. The stratum of atmospheric air between the two boxes was, as already stated, 2326 metres in thickness.

Our experiments commenced on the morning of the 8th of September 1864, the sky being admirably pure and the air perfectly calm. Every hour from sunrise to 10 in the morning, and every half hour from the latter period to 3 in the afternoon, we observed, *first*, the thermometer at the surface of the willow soil; *secondly*, the thermometer which was imbedded therein; *thirdly*, a thermometer with a small bulb placed in the shade and previously well swung (*tourné en fronde*); and *lastly* a psychrometer, also placed in the shade. The sky at Bagnères remained constantly pure, and I was enabled to make all the observations agreed upon. It was otherwise on the Pic du Midi; the mornings were there magnificent, but towards noon *cirro-cumuli* rose from

* *Annuaire de la Société Météorologique de France*, 1863, vol. xi. p. 129; and *Mémoires de l'Acad. des Sciences de Montpellier*, 1863, vol. v. p. 374.

the Spanish side, and, driven by a slight breeze from the south, gradually enveloped the Pic. Nevertheless during the three days I was able to select twenty corresponding and perfectly comparable observations, made alike under a pure sky, in full sunlight and in calm air: these coincidences occurred between 7 in the morning and 2 in the afternoon.

With reference, *first*, to the heating of the superficial soil, the mean of the twenty observations on the temperature of the air in the shade was 22°·3 at Bagnères, and only 10°·1 at the Pic du Midi. The mean temperature of the surface of the soil was 36°·1 at Bagnères, and 33°·8 at the Pic. The mean excess of the temperature of the soil above that of the air at Bagnères had consequently to the like excess at the Pic the ratio of 10 to 17; in other words, it was nearly double on the mountain. Further, although the mean temperature of the soil in the plain was 2°·3 above that of the soil on the mountain; still, on the 10th of September, between 11 and 11.30 A.M. (after which latter time the Pic became enveloped in clouds), the temperature of the soil at its summit was 6°·9 higher than that of the soil at Bagnères—the mean temperature of the air at the latter station being 23°·2, and at the former 13°·8. The absolute maxima observed at the surface of the soil in the twenty corresponding observations were 50°·3 at Bagnères (2 P.M., September 9, temperature of air in the shade 27°·1), and 52°·3 on the Pic du Midi (11.30 A.M., September 10, temperature of air in the shade 13°·2). These experiments place beyond doubt the fact of the greater calorific power of the sun on the mountain than in the plain.

I proceed, *secondly*, to the heating of the soil at a depth of 5 centimetres. I have already stated that the soil resulting from the decomposition of the wood of the willow was penetrated with greater difficulty by solar heat than was any one of the other soils I examined, and besides this that it was the best radiator. The physicist will not be at all surprised at this. The greater relative heating power of the sun on the mountain is shown by the thermometers whose bulbs were at a depth of 0·05 of a metre, as well as by those whose bulbs rested on the surface. Thus the mean temperature of the soil at a depth of 0·05 of a metre was 25°·5 at Bagnères, that is to say, only 3°·2 above the temperature of the air, whilst on the Pic du Midi the first temperature was 17°·1, higher consequently by 7 degrees than the temperature of the air surrounding this elevated summit. The mean thermic excess, at Bagnères and at the Pic, of the soil at a depth of 0·05 of a metre, is therefore as 10 : 22; that is to say, on the mountain the excess in question is more than double that in the plain. On the surface of the soil the ratio, as we have seen, was as 10 to 17. Relatively, therefore, the

soil at a few centimetres below the surface is still more heated on the mountain than it is in the plain.

This immense heating of the soil, compared with that of the air on high mountains, is the more remarkable, since during the nights the cooling by radiation is there much greater than in the plain. I have already had the honour of communicating to the Academy (on the 6th of June, 1859) the results obtained by MM. Peltier, Bravais, and myself, by means of Pouillet's swans-down actinometer, on the summit of the Faulhorn, at an altitude of 2683 metres, and on the Grand Plateau below Mont Blanc, at 3930 metres above the level of the sea. I afterwards developed the same subject in a memoir on the causes of the low temperatures on high mountains*. Two corresponding observations of the minima of the air and of the soil at Bagnères and at the Pic du Midi during the clear nights from the 7th to the 8th, and from the 8th to the 9th of September, will show still more strikingly how much more considerable is the cooling of the soil by radiation in the rarefied air of high mountains than in the denser strata of plains. The willow soil being, of all those which I tested, the one whose emissive power is greatest, the low temperatures given in the sixth column of the following Table, containing the results we obtained, will cause no surprise.

Lowest Temperatures of the Air and of the Soil during the Night.

Dates, September 1864.	Bagnères, altitude 551 metres.			Pic du Midi, altitude 2877 metres.			Remarks.
	Lowest temperature			Lowest temperature			
	Of the air.	Of the soil at a depth of 0·00 metre.	Of the soil at a depth of 0·25 metre.	Of the air.	Of the soil at a depth of 0·00 metre.	Of the soil at a depth of 0·25 metre.	
8	14°·4	10°·5	13°·8	° ...	−2°·8	2°·6	Hoar-frost on willow soil. Dewon the soil of the Pic.
9	14·9	12·9	13·9	1·3	0·1	2·1	

It will be seen that the difference between the lowest temperatures at the surfaces of the soil was 13°·3 during the night from the 7th to the 8th of September, and 12°·8 during that from the 8th to the 9th, the excess being in favour of the plain. When the sun rises, his oblique rays impart heat, very slowly at first, to the frozen soil of high mountains: thus, although his rays had already struck the summit of the Pic at 5.30 A.M., it was not until 7 A.M. that the surface of the soil reached a mean temperature of 9°·3; at 11.30 A.M. this tempe-

* *Annales de Chimie et de Physique*, 3 sér. vol. lviii. (1860).

rature had risen to 47°·8. Not being in possession of corresponding afternoon observations, I am not able to fix the time of greatest temperature; I am inclined to think, however, that, as in the case of the air, it occurred about noon.

Ramond, in the thirty-five ascents of the Pic du Midi made by him in the course of twelve years in spring, summer, and winter, collected all the plants of the terminal cone, whose height is 16 metres, and whose base covers several ares; he there observed 71 phanerogamous plants. I noticed 131 such plants on the terminal cone of the Faulhorn, whose height is 80 metres, and whose base covers 4 hectares 50 ares (about 11 English acres), the summit being 2683 metres above the level of the sea. According to the recent Swedish explorations, and the account of previous expeditions given by M. Malmgren*, the whole archipelago of Spitzbergen only contains 93 such plants. Independently of the original geographical distribution, the temperature of the soil is sufficient to account for the number and the variety of the species which vegetate on the summits of the Alps and of the Pyrenees; they are there heated more by the soil which bears, than by the air which surrounds them, and their respiratory functions are stimulated by a strong light. On the other hand, at Spitzbergen, notwithstanding the continual presence of the sun above the horizon during summer, the heat of the sun's rays, absorbed almost entirely by the vast thickness of atmosphere they have traversed, is incapable of raising the temperature of the soil above that of the air. The soil remains always frozen at a depth of a few decimetres; and vegetation not being stimulated either by the heat of the air or by that of the soil, the entire flora of the region is limited to a small number of plants capable of living and flourishing at a temperature but a few degrees above zero.

III. *On a case of Stereoscopic Illusion.* *By* C. J. Monro†.

IF the phenomenon here to be described has not been described before, it is perhaps worth describing for its own sake. If it is well known, it is worth while to point out its bearing on a recently contested point in the theory of binocular vision.

In his separate work on the Stereoscope, published in 1856, Sir David Brewster maintained (pp. 78, 81) that when we see or seem to see an object single and solid with two eyes, it is because we see any point with both eyes at the intersection of the two

* Petermann's *Mittheilungen*, 1863, p. 48.

† Communicated by the Author.

directions in one of which we see it with each eye. Mr. Abbott* rejects this as an explanation (pp. 106, 107), and denies it as a fact (p. 107), but is not prepared to give a construction of his own for the apparent place of any point whose images on the two retinas are not similarly situated with reference to the yellow spots. The apparent places of all points having this property he determines, as he does the apparent directions of all others, by constructions which only relate to the case of an observer looking straight before him, but, at least for the former class of points, are the same as Sir David's as far as they go (pp. 108, 109). Indeed Mr. Abbott would probably admit that Sir David's construction was approximately accurate as a geometrical statement of the facts of ordinary vision. That, on the contrary, it does not in general meet the case of that artificial vision which formed the subject of Sir David's book, Mr. Abbott has perhaps partly proved†; and the demonstration is completed by the fact that when stereoscopic pictures further apart than the eyes are combined by divergence, the object is given in relief as perfect as that of nature, and does not, in my experience, appear behind the observer. But even in artificial vision, as far as my own eyes are concerned, the theory seems to apply correctly to all cases which, according to that theory, would suggest an idea easily adopted by the imagination. Strange to say, in the simplest of these cases, whereas my experience agrees with Sir David's theory, Sir David's experience seems to contradict it.

In describing the effect of combining an image of each section of a repeated pattern with the conjugate image of another section a certain distance off, he says (Stereoscope, p. 91) "the surface .... seems slightly curved." To Mr. Abbott, who seems to speak, like Sir David, of an apparent surface formed on this side of the real one, it looks "somewhat convex" (Sight and Touch, p. 122). Now if the eyes are taken to be represented by their optical centres, the theory would evidently give a flat surface,

* Sight and Touch, an attempt to disprove the received or Berkleian Theory of Vision. By T. K. Abbott, Fellow of Trinity College, Dublin (1864).

† He gives a pair of dots in the middle of the page (107), and says that on combining them stereoscopically we do not see the image below or above the paper, but upon it. Now, in the first place, I do see the image below or above the paper according as I combine the pair directly or conversely. Secondly, the proof meets only half of the proposition. It shows that Sir David's conditions are insufficient under very trying circumstances: it does not show that they are unnecessary. I venture to use the phrases *direct* and *converse* stereoscopic combination, for which I find no equivalents in either of the works cited. The combination is direct when the right eye sees the right picture, and the left the left; converse when the right eye sees the left picture, and the left the right.

and I presume the error due to this substitution would be something quite imperceptible to the lateral parts of the retina. The eyes are converged indeed upon points very near them, and focused for points further off; and though the disproportionate recession of the optical centres and advance (if there is any advance) of the retina due to this inconsistency would affect all points of the picture alike, it is intelligible, on Mr. Abbott's principles, that a flattening of the retina, apparently inseparable from its advance, might produce the effect of convexity by drawing lateral images a little nearer to the yellow spots. But any illusion depending upon the properties of lateral parts of the retina would disappear upon running the eyes rapidly over the field. Meanwhile, having repeatedly made the experiment both before and after meeting with Sir David's observations, I have never detected the slightest curvature in the apparent surface, except in two cases—first when the real surface is curved, and secondly when the eyes are at different distances from it. In the cases described the real surface is a flat wall, and the observers are evidently supposed to be looking straight at it: besides, if they had looked obliquely the curvature would have been concave, according to the theory; for the surface the theory gives is a hyperbolic cylinder asymptotic to the wall.

It is difficult, if not impossible, to see this in patterns which vary vertically as well as horizontally; but if the pattern consists simply of vertical lines, nothing can be more vivid than the object sometimes suggested. I combined in this way the images of a straight row of flat vertical bars, about an inch and a half wide and an inch and a half apart, so as to form a stereoscopic image beyond them: my nearest eye was about six inches from their plane, to which the line joining the two was inclined at a considerable angle. The bars immediately formed themselves into the alternate faces of a vertical prism, of which the circumscribing cylindrical surface was apparently asymptotic to the plane of their real position, and came up to a sort of apse quite close to me, and then stretched off indefinitely away from the plane as far as the binocular field extended. The sudden changes of azimuth made by the flat surfaces as they turned the corner of greatest curvature gave the image an effect of solidity as perfect as that of real nature. Now every test of distance and position, except that of the intersection of the visual rays, was against the production of the result produced. The very points which the focusing of the eyes affirmed to be the nearest stretched away in the further sheet of the surface; and while the nearly equal illumination of the bars by a single lamp affirmed that they were parallel, they seemed to stand round a very convex surface. So even without positive measurement, which seems impossible,

and without comparison of estimated with calculated results, which it would be very hard to conduct satisfactorily, I think I may give this phenomenon as a confirmation of Sir David Brewster's view, subject to the qualification above stated, which he applies himself in a particular class of cases at pages 209, 210, and 217.

That such a surface is given by the theory is evident. The visual rays approach without limit to parallelism as you run your eyes away along the wall: they are parallel also when the four quantities following are proportional, the distances of the eyes and of the points to be combined from the intersection of the wall by the line through the eyes. When these are proportional to one another, they are proportional to their differences. So the direction of the second asymptote is that of the base of a triangle whose sides are in direction and magnitude the distances between the eyes and between the points to be combined. The latter distance is in these patterns constant in magnitude, and must be positive if the stereoscopic combination is direct, and negative if converse, provided that the directions containing the acute angle are taken to be of the same sign. The surface is cylindrical, because the points to be combined in any part of the pattern, and therefore the lines through them from fixed points, and the intersection of these, all have for orthogonal projections points and lines answering the same description in the horizontal plane.

For further details, let us confine our attention to this plane. Take for axes the line through the eyes and the trace of the wall; and, with the above convention as to signs, let the distances of the eyes and of the two other points from the origin be a, a', b, b'. Then, if it is remembered that a, a' and $b-b'$ are constant, the coordinates of the point of intersection of the visual rays will be given by the equations

$$\frac{x}{a}+\frac{y}{b}=1, \quad \frac{x}{a'}+\frac{y}{b'}=1;$$

whence

$$x^2+\frac{a-a'}{b-b'}xy-(a+a')x+aa'=0,$$

the equation of an hyperbola of whose asymptotes the equations are

$$x=0, \quad \frac{x}{a-a'}+\frac{y}{b-b'}=\frac{a+a'}{a-a'}.$$

The line given by the last equation cuts the line through the eyes as far on one side of the mid-point between them as the trace of the wall cuts it on the other, and makes with these lines

a triangle similar and similarly situated to that described above without the help of an equation.

In the case of direct combination, the visible branch is beyond the wall, and is convex to the observer; the other branch passes through the eyes; and a short arc, generally near the apse, is in front of them, but, being formed by combining points of which one is concealed from the corresponding eye, is invisible. The rest is behind the observer, and I suppose none of it can be seen in its proper place; but a small part near the second asymptote is formed by combining points, both of which may be visible, and under favourable circumstances might be combined by divergent visual rays. Where the image would appear, is a curious question.

In the case of converse combination, the visible part is on this side of the wall, and is of course concave to the observer. The curve runs up to the nearest eye, and, though it comes out at the other, is seen no more of.

Gibraltar, November 1864.

IV. *Note on a Quartic Surface.* *By* A. Cayley, *F.R.S.**

IT would, I think, be worth while to study in detail the quartic surface which is the envelope of a sphere having its centre on a given conic, and passing through a given point. The equations of the conic being $z=0$, $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$, the coordinates of a point on the conic may be taken to be $x=a\cos\theta$, $y=b\sin\theta$, $z=0$, whence, if (α, β, γ) be the coordinates of the given point, the equation of the sphere is

$$(x-a\cos\theta)^2+(y-b\sin\theta)^2+z^2=(\alpha-a\cos\theta)^2+(\beta-b\sin\theta)^2+\gamma^2,$$

or, what is the same thing,

$$x^2+y^2+z^2-\alpha^2-\beta^2-\gamma^2-2(x-\alpha)a\cos\theta-2(y-\beta)b\sin\theta=0;$$

and hence the equation of the surface is at once seen to be

$$(x^2+y^2+z^2-\alpha^2-\beta^2-\gamma^2)^2=4a^2(x-\alpha)^2+4b^2(y-\beta)^2.$$

If $a=b$ (that is, if the conic be a circle), then we may without loss of generality write $\beta=0$, and the equation then is

$$(x^2+y^2+z^2-\alpha^2-\gamma^2)^2=4a^2\{(x-\alpha)^2+y^2\}.$$

This may be written

$$(x^2+y^2+z^2-\alpha^2-\gamma^2-2a^2)^2=-8a^2\alpha\left(x-\frac{a^2+2\alpha^2+\gamma^2-z^2}{2\alpha}\right),$$

* Communicated by the Author.

which, considering z as a constant, is of the form

$$(x^2+y^2-\alpha)^2=16\mathrm{A}(x-m);$$

that is, the section of the surface by a plane parallel to the plane of the conic is a Cartesian.

If a and b are unequal, but if we still have $\beta=0$, the equation of the surface is

$$(x^2+y^2+z^2-\alpha^2-\gamma^2)^2=4a^2(x-\alpha)^2+4b^2y^2.$$

There are here two planes parallel to the plane of the conic, each of them meeting the surface in a pair of circles. In fact, writing $x^2+y^2=\rho$, and therefore also $y^2=\rho-x^2$, putting moreover $z^2-\alpha^2-\gamma^2=k$, we have

$$(\rho+k)^2=4a^2x^2-8a^2\alpha x+4a^2\alpha^2+4b^2(\rho-x^2);$$

that is,

$$\rho^2+4(b^2-a^2)x^2+k^2-4a^2\alpha^2+8a^2\alpha x+(2k-4b^2)\rho=0,$$

or, as this may also be written,

$$(1, 4(b^2-a^2),\ k^2-4a^2\alpha^2,\ 4a^2\alpha,\ k-2b^2,\ 0\mathcal{X}\rho,\ x,\ 1)^2=0,$$

which is of the form

$$(a,\ b,\ c, f, g,\ 0\mathcal{X}\rho,\ x,\ 1)^2=0;$$

and the left-hand side will break up into factors, each of the form $\rho+\mathrm{A}x+\mathrm{B}$ (so that, equating either factor to zero, we have $\rho+\mathrm{A}x+\mathrm{B}=0$, that is, $x^2+y^2+\mathrm{A}x+\mathrm{B}=0$, the equation of a circle), if only

$$abc-af^2-bg^2=0.$$

Writing this under the form $b(ac-g^2)-af^2=0$, and substituting for a, b, c, f, g their values, we have

$$b=4(b^2-a^2),\ ac-g^2=k^2-4a^2\alpha^2-(k-2b^2)^2=4(b^2k-b^4-a^2\alpha^2),$$
$$af^2=16a^4\alpha^2,$$

and therefore the condition is

$$(b^2-a^2)(b^2k-b^4-a^2\alpha^2)-a^4\alpha^2=0;$$

that is,

$$b^2\{(b^2-a^2)(k-b^2)-a^2\alpha^2\}=0.$$

If $b^2=0$, the surface is a pair of spheres; rejecting this factor, we have $(b^2-a^2)(k^2-b^2)-a^2\alpha^2=0$; or putting for k its value, the condition becomes

$$(b^2-a^2)(z^2-\alpha^2-\gamma^2-b^2)-a^2\alpha^2=0;$$

that is, for each of the values of z given by this equation, the section by a plane parallel to the plane of the conic will be a pair of circles.

The planes in question will coincide with the plane of the conic, if only

$$(b^2-a^2)(\alpha^2+\gamma^2+b^2)+a^2\alpha^2=0,$$

or, what is the same thing,

$$b^2\alpha^2-(a^2-b^2)\gamma^2=b^2(a^2-b^2);$$

that is, if the point $(\alpha, 0, \gamma)$ be situated on the hyperbola $y=0$, $\frac{x^2}{a^2-b^2}-\frac{z^2}{b^2}=1$. The hyperbola in question and the ellipse $z=0$, $\frac{x^2}{a^2}+\frac{y^2}{b^2}=1$, are, it is clear, conics in planes at right angles to each other, having the transverse axes coincident in direction, and being such that each curve passes through the foci of the other curve; or, what is the same thing, they are a pair of focal conics of a system of confocal ellipsoids.

The surface in the case in question, viz. when the parameters a, b, α, β are connected by the equation

$$\frac{\alpha^2}{a^2-b^2}-\frac{\gamma^2}{b^2}=1,$$

is in fact the "Cyclide" of Dupin. It is to be noticed that we have here

$$\begin{aligned}&(\alpha-a\cos\theta)^2+b^2\sin^2\theta+\gamma^2\\&\quad=\alpha^2+\gamma^2+b^2-2a\alpha\cos\theta+(a^2-b^2)\cos^2\theta;\end{aligned}$$

which, observing that $\alpha^2+\gamma^2+b^2$ is $=\frac{a^2\alpha^2}{a^2-b^2}$, gives

$$(\alpha-a\cos\theta)^2+b^2\sin^2\theta+\gamma^2=\left(\sqrt{a^2-b^2}\cos\theta-\frac{a\alpha}{\sqrt{a^2-b^2}}\right)^2;$$

so that the radius of the variable sphere is

$$=\sqrt{a^2-b^2}\cos\theta-\frac{a\alpha}{\sqrt{a^2-b^2}}.$$

If the variable sphere, instead of passing through the point $(\alpha, 0, \gamma)$ on the hyperbola, be drawn so as to touch a sphere of radius l, having its centre at the point in question, then the radius of the variable sphere would be

$$\sqrt{a^2-b^2}\cos\theta-\frac{a\alpha}{\sqrt{a^2-b^2}}-l,$$

which is in fact

$$=\sqrt{a^2-b^2}\cos\theta-\frac{a\alpha'}{\sqrt{a^2-b^2}},$$

if only $\alpha' = \alpha + \frac{b\sqrt{a^2-b^2}}{a}$; hence if γ' be the corresponding value of γ, the variable sphere passes through the point $(\alpha', 0, \gamma')$ on the hyperbola, and the envelope is still a cyclide. The cyclide as derived from the foregoing investigation is thus the envelope of a sphere having its centre on the ellipse, and touching a fixed sphere having its centre on the hyperbola. It also appears that there are, having their centres on the hyperbola, an infinite series of spheres each touched by the spheres which have their centre on the ellipse; if, instead of one of these spheres we take any four of them, this will imply that the centre of the variable sphere is on the ellipse, and it is thus seen that the cyclide as obtained above is identical with the cyclide according to the original definition, viz. the envelope of a sphere touching four given spheres.

Cambridge, December 5, 1864.

V. *On the approximate Graphic Measurement of Elliptic and Trochoidal Arcs, and the Construction of a Circular Arc nearly equal to a given Straight Line.* *By* W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L. & E.**

[With a Plate.]

THE three following rules are very obvious results of the application of Simpson's method of approximate integration to the rectification of curves generated by rolling; and it is possible that they may have been already published by other authors; but as I do not know of any such publication, and as the rules are useful and convenient, I beg leave to offer them to the Philosophical Magazine†.

The general proposition of which the rules are particular cases is the following well-known one. Let a plane disk of any figure roll on a plane base-line of any figure: let there be a tracing-point in the disk, and let r denote the *rolling radius*, or distance of the tracing-point at any instant from the instantaneous centre, or point of contact of the disk and base-line. Then while the disk rolls through the angle ϕ, the tracing-point describes an arc of the length $\int_0^\phi r d\phi$. To calculate an approximate value of this integral, divide the angle ϕ into either $2n$ or $3n$ equal inter-

* Communicated by the Author.

† A process nearly identical with that of Rule I. is applied by John Bernoulli to the rectification of the *whole ellipse*, but not to elliptic arcs. (*Johannis Bernoullii Opera*, vol. i. § 83.)

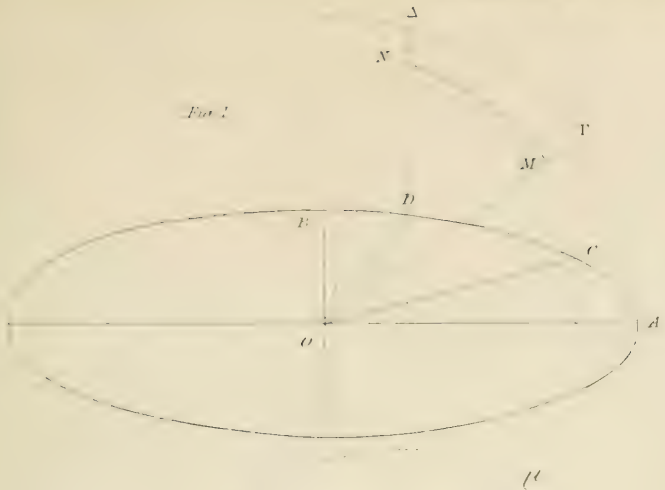


Fig. 1.

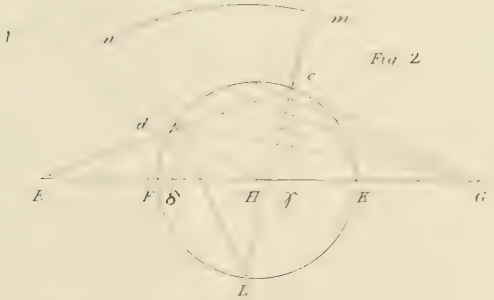


Fig. 2.



Fig. 3.

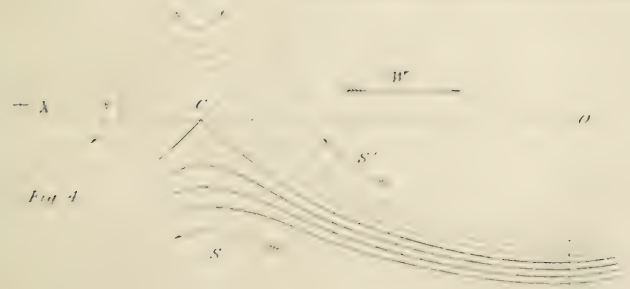


Fig. 4.

vals (the number being the greater the closer the required approximation). Measure the rolling radii corresponding to 0 and ϕ, and to each of the intermediate angles, and let them be denoted by r_0, r_1, r_2, &c. Then the *mean rolling radius* is approximately for $2n$ intervals (by Simpson's first rule)

$$r_m = \frac{1}{6n}(r_0 + 4r_1 + 2r_2 + 4r_3 + \ldots + 2r_{2n-2} + 4r_{2n-1} + r_{2n});$$

and for $3n$ intervals (by Simpson's second rule),

$$r_m = \frac{1}{8n}(r_0 + 3r_1 + 3r_2 + 2r_3 + \ldots + 2r_{3n-3} + 3r_{3n-1} + 3r_{3n-1} + r_{3n});$$

and the arc described by the tracing-point is approximately equal to *a circular arc of the radius r_m, subtending the angle ϕ.*

Rule I. To construct a circular arc approximately equal to a given arc C D (Plate I. fig. 1), not exceeding a quadrant, of an ellipse whose semiaxes O A and O B are given.

In fig. 2, draw a straight line, in which take E F = O B and F G = O A. Bisect it in H; and about that point, with the radius $H F = H K = \frac{OA - OB}{2}$, describe a circle. Mark the points *c* and *d* in that circle, by laying off E*c* = OC, and E*d* = OD.

Then divide the arc *cd* into $2n$ or $3n$ equal intervals, as the case may be, and measure the distances from the ends of the arc and the points of division to G: these will be rolling radii of the ellipse, as generated by rolling a circle of the diameter E H inside a circle of the diameter E G, the tracing-point being at the distance H F from the centre of the rolling circle; and the *Simpsonian mean* (as it may be called) of those rolling radii will be *the radius of the required circular arc.*

Then in fig. 1 describe a circle about O with the radius O A; through C and D draw straight lines parallel to O B, cutting that circle in Γ and Δ; join O Γ, O Δ; and about the centre O, with the mean rolling radius already found, describe the circular arc M N, bounded by the straight lines O Γ, O Δ; this will be *the required circular arc approximately equal to the elliptic arc* C D.

The same circular arc may be drawn, if required, in fig. 2 as follows:—From any convenient point L in the circumference of the circle F K, draw straight lines through *c* and *d*; and about L as a centre, with the mean rolling radius already found, describe the circular arc *m n* bounded by those lines.

Rule II. *To construct a circular arc approximately equal to a given trochoidal arc, not exceeding the arc between a crest of the trochoid and the adjoining hollow.*

In fig. 2 make G H = the radius of the rolling circle which

generates the trochoid; and describe the small circle F K about H with a radius equal to the distance of the tracing-point from the centre of the rolling circle. Take Gγ and Gδ respectively equal to the perpendicular distances of the two ends of the given trochoidal arc from the base-line on which the rolling circle rolls. Through γ and δ draw straight lines perpendicular to G H, cutting the small circle in *c* and *d*. Divide the arc *c d* into $2n$ or $3n$ equal intervals, and measure the distances from the ends of the arc and the points of division to G; these will be rolling radii; and their Simpsonian mean will be *the radius of the required circular arc.*

From H draw straight lines through *c* and *d*, and about H, with the radius already found, describe the circular arc $\mu\nu$, bounded by these straight lines: this will be *the required circular arc approximately equal to the given trochoidal arc.*

Rule III. To find the radius of a circular arc which shall be approximately equal to a given straight line, and shall subtend a given angle.

This question is solved by regarding the straight line as forming part of an ellipse whose shorter semiaxis is=0. Let P Q (fig. 3) be the given straight line. Construct the triangle P Q R, in which Q is a right angle, and P the complement of the given angle; so that R is the given angle itself. Bisect the hypothenuse P R in S, about which point describe a circular arc traversing P and Q. Divide that arc into $2n$ or $3n$ equal intervals: measure the distances from its ends and from the points of division to the point R, and take their Simpsonian mean; this will be *the radius* R T *of the required arc* T U, *which is approximately equal to the straight line* P Q, *and subtends the angle* P R Q.

Remarks.—It is evident that the processes described in the three preceding rules are graphic methods of approximating to elliptic functions of the second kind, and that, if put in an algebraical form, they would become identical with the method of approximation to the function E described by Legendre in the Appendix to the first volume of his *Traité des Fonctions Elliptiques.*

The *amplitude* of the function is represented by

$$\phi = \text{MON of fig. 1,}$$

$$= m\text{L}n = \frac{\mu\text{H}\nu}{2} \text{ of fig. 2,}$$

$$= \text{QRP of fig 3.}$$

The *modulus* in fig. 1 is the eccentricity of the ellipse, in fig. 2

$\frac{\sqrt{FG^2-KG^2}}{FG}$, and in fig. 3 unity; and the unit line in fig. 1 is OA, in fig. 2 F G, and in fig. 3 P R.

The degree of precision of the approximation depends on the smallness of the intervals into which the amplitude is divided—the error diminishing somewhat faster than the fourth power of the interval, as Legendre, in the Appendix already referred to, has shown to be the case for every application of Simpson's formula. Thus, in the application of Rule III., the following are examples of the greatest proportionate errors (which are always in excess):—

Intervals of amplitude.	Errors of Rule III.
45°	about $\frac{1}{400}$
30	„ $\frac{1}{2500}$
$22\frac{1}{2}$	„ $\frac{1}{8000}$
15	„ $\frac{1}{48000}$

The errors of Rules I. and II. become smaller than those of Rule III. as the modulus diminishes—that is, as the ellipse approaches to a circle, and the trochoid to a straight line. The following are examples of the errors of Rule I., in the length of a circular arc equal to an elliptic quadrant, the major semiaxis being unity:—

Eccentricity.	True length (from Legendre's Tables).	Approximate length by Rule I.			
		Two intervals of 45°.	Errors.	Three intervals of 30°.	Errors.
$\sqrt{\frac{1}{2}}$	1·3506	1·3538	·0032	1·3520	·0014
0·6	1·4184	1·4195	·0011	1·4186	·0002
0·5	1·4675	1·4681	·0006	1·4678	·0003

Glasgow University,
November 19, 1864.

VI. *Supplement to a Paper on Stream-lines*.*

By W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L. & E.*

[With a Plate.]

27. THE following is a demonstration of the proposition, stated in an Addendum to the previous paper, that *all waves in which molecular rotation is null, begin to break when the two slopes of the crest meet at right angles*:—

The profiles of the layers of a series of waves are converted, as is well known, into undulating stream-lines by supposing

* In the Philosophical Magazine for October 1864.

the actual motion of the particles of water to be combined with a uniform translation equal and opposite to the velocity of propagation of the waves.

Thus, let $-c$ be the velocity of the waves propagated in a *negative* direction, as indicated by the arrow W in Pl. I. fig. 4; and, as in article 13 of the original paper, let $c(u-1)$ and cv be the horizontal and vertical components of the actual velocity of a particle of water in the waves, so that cu and cv shall be the components of the velocity of the same particle in the undulating stream into which the waves are converted, and which flows in a *positive* direction, as indicated by the arrows S, S′.

The *tangent of the slope* of a stream-line or wave-line at any point is obviously $\frac{v}{u}$.

The *crest* of a wave-line is a point where $v=0$ and u is a minimum. The latter property distinguishes it from the trough, where $v=0$ also, but u is a maximum.

The *equations of a crest* are as follows:—

$$v=0; \quad \frac{du}{dt}=0.$$

A wave begins to *break* so soon as its crest ceases to be rounded and becomes angular, as at C in fig. 4; and at such a point it is evident that u must vanish as well as v. The ratio of two quantities which vanish simultaneously is equal to the ratio of their differentials. Hence *the equations of a sharp crest,* in addition to the equations (I.) and (II.), are the following:—

$$u=0, \quad \frac{\frac{dv}{dt}}{\frac{du}{dt}}=\frac{v}{u}.$$

By putting for $\frac{d}{dt}$ the equivalent operation $cu\frac{d}{dx}+cv\frac{d}{dy}$, the preceding equations are transformed into the following.

At every crest

$$v=0, \quad \ldots\ldots\ldots \quad \text{(I.)}$$

$$u\frac{du}{dx}+v\frac{du}{dy}=0; \quad \ldots\ldots\ldots \quad \text{(II.)}$$

and at every sharp crest

$$u=0, \quad \ldots\ldots\ldots \quad \text{(III.)}$$

$$\frac{u\frac{dv}{dx}+v\frac{dv}{dy}}{u\frac{du}{dx}+v\frac{du}{dy}}=\frac{v}{u} \quad \ldots\ldots \quad \text{(IV.)}$$

Equation (IV.) may be reduced to the following form:—

$$\frac{dv}{dx}+\frac{v}{u}\left(\frac{dv}{dy}-\frac{du}{dx}\right)-\frac{v^2}{u^2}\cdot\frac{du}{dy}=0. \quad \text{(IV. A)}$$

The well-known *equation of continuity* is

$$\frac{du}{dx}+\frac{dv}{dy}=0, \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(V.)}$$

which reduces (IV. A) to the following:

$$\frac{dv}{dx}-2\frac{du}{dx}\cdot\frac{v}{u}-\frac{du}{dy}\cdot\frac{v^2}{u^2}=0, \quad . \quad . \quad . \quad \text{(IV. B)}$$

whose two roots are as follows:

$$\frac{v}{u}=-\frac{\frac{du}{dx}}{\frac{du}{dy}}\pm\sqrt{\left\{\frac{\frac{dv}{dx}}{\frac{du}{dy}}+\frac{\frac{du}{dx^2}}{\frac{du^2}{dy^2}}\right\}}; \quad . \quad . \quad \text{(VI.)}$$

and those two roots are the *tangents of the slopes* that meet at the crest.

It is known that in a perfect fluid the quantity known as the *molecular rotation*, or $\frac{c}{2}\left(\frac{du}{dy}-\frac{dv}{dx}\right)$, is either nothing or a constant for each stream-line. The present proposition refers to the case in which

$$\frac{du}{dy}-\frac{dv}{dx}=0; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(VII.)}$$

and consequently the tangents of the two slopes are the two values of the expression

$$\frac{v}{u}=-\frac{\frac{du}{dx}}{\frac{du}{dy}}\pm\sqrt{\left\{1+\frac{\frac{du^2}{dx^2}}{\frac{du^2}{dy^2}}\right\}}; \quad . \quad . \quad \text{(VIII.)}$$

but the product of those two values is $=-1$; therefore *at every sharp or breaking crest of a wave in which there is no molecular rotation, the two slopes meet each other at right angles.* Q. E. D.

28. The preceding demonstration applies to all waves whatsoever in which molecular rotation is null. In waves which, besides having that property, are symmetrical at either side of the crest, each of the slopes which meet at the crest of a wave on the point of breaking is inclined at 45° to the horizon.

29. The figure (which is reduced from a diagram exhibited at the late Meeting of the British Association) represents one-half of a forced wave, such as has been described in articles 13 and

14 of the previous paper. At C is a right-angled crest; and the full line traversing C is that for which $b=1$. The dotted continuations of the same line above C satisfy the same equation, but do not belong to any possible wave-surface. The full lines below C are wave-lines for which b has a series of values greater than 1, viz. $1\frac{1}{12}$, $1\frac{2}{12}$, $1\frac{3}{12}$, &c.; and the dotted curves above C fulfil the same equations, but are not possible wave-lines. The dotted lines to the right and left of C are stream-lines for which b has values less than 1, such as $\frac{11}{12}$, $\frac{10}{12}$, $\frac{9}{12}$, &c., and are not continuous wave-lines.

Glasgow University,
November 19, 1864.

VII. *On Calcescence.* *By* Dr. C. K. Akin*.

1. *RADIESCENCE.*—The radiescent state of substances is known to originate in three different modes, which may be distinguished by the terms Spontaneous Radiation, Production, and Reproduction of Rays. As spontaneous radiation may be designated all those phenomena of ray-emission to which, apparently, no immediate cause can be assigned, and which, in the last instance, are probably owing to certain velocities impressed on the molecules of matter from all beginning, together with certain intermolecular relations, corresponding in some degree to the primitive tangential tendency and attractive forces which sustain the motions of the solar and planetary systems. Under production of rays may be comprised all those phenomena of radiation which are engendered by the agency of finite movement, affinity, cohesion, or electricity; whilst under reproduction those instances of radiation may be classed which arise from the incidence or communication of rays derived from distant or contiguous sources.

2. *Renovation.*—The reproduction of rays derived from distant sources, to which it is wished generally to call attention in this paper, may take place under circumstances of a double nature, and is termed accordingly Diffusion (or Reflexion) and Renovation—the term diffusion being common and well known, and that of renovation introduced here with a view to avoid future ambiguity. The important distinction which underlies these terms may be best understood from the language employed by Dr. Young for its elucidation (or rather for some more general

* Being the abstract of two papers read before the Mathematical and Physical Section of the British Association at Newcastle, in August and September 1863. Reprinted, with additions and verbal corrections, from No. 39 of the 'Reader' (September 26, 1863). N.B. Paragraphs newly added will be enclosed within brackets.

purpose):—"It seems highly probable," he says*, "that light and heat occur to us each in two predicaments, the vibratory or permanent, and the undulatory or transient state—vibratory light being the minute motions of ignited bodies, or of solar phosphori, and undulatory or radiant light the motion of the æthereal medium excited by these vibrations—vibratory heat being a motion to which all material substances are liable, and which is more or less permanent, and undulatory heat that motion of the same æthereal medium, which has been shown by Mr. King and M. Pictet to be as capable of reflexion as light, and by Dr. Herschel to be capable of separate refraction." The distinction thus clearly drawn between the separate offices of matter and æther, with reference to radiation upon the whole, is particularly applicable to those which regard the phenomena of ray-reproduction—the "undulatory or transient" reproduction, or, in current parlance, diffusion, being attributable to the agency of æther, and the "vibratory or permanent" reproduction, or, as designated above, renovation, to the intervention of matter.

3. *Transmutation.*—Of the many phenomena of nature which belong to the domain of renovation, only two (or, rather, one only) have been hitherto investigated—namely, fluorescence and the cognate phenomenon of phosphorescence. Fluorescence may be defined as a case of renovation in which the emitted rays belong to the order of the visible, and the incident, which are the cause of the emitted, either to the same order, or to a higher order, as regards refrangibility, and where emission apparently ceases with incidence; while phosphorescence has to be considered as a case of fluorescence, distinguished from ordinary fluorescence by a sensible protraction of emission beyond the duration of incidence†.

The phenomena of fluorescence, if not of phosphorescence, are specially interesting from their having evidenced the change of refrangibility to which rays are liable in the act of renovation—visible rays having been transmuted by the agency of fluorescent matter into visible rays of different colour, and invisible rays actually transmuted into visible rays. As will presently be shown, however, the range of possible transmutations has been hitherto far from exhausted; and besides the transmutations observed in fluorescence, a number of others seem *à priori* capable of being effected, some of which it would be highly interesting to realize. For this purpose it is first necessary to consider the constitution of the spectrum of a solar or other similar beam of rays. Any such spectrum, as is well known,

* Phil. Trans. for 1802, p. 47.

† See English Cyclopædia (Arts and Sciences), vol. iv. p. 124.

consists of three compartments, of which the median takes in the visible rays, and the two others, respectively, the rays of greater refrangibility and of less refrangibility than corresponds to visible or light-rays. In order to avoid circumlocution, and the ambiguity which attaches to the terms actually in use, it is proposed to adopt in the sequel the following nomenclature for the above-mentioned three classes of rays. The visible rays will be called Newtonic, those of greater refrangibility Ritteric, and finally those of smaller refrangibility Herschellic—the name being formed in each case from that of the discoverer of the particular class of rays. By means of this nomenclature it is easy to give a complete list of the transmutations of rays *à priori* possible, and which accordingly is here appended:—

Transmutations.

(1) Of Ritteric rays into Ritteric rays of less refrangibility.
(2) Of Ritteric rays into Newtonic rays.
(3) Of Ritteric rays into Herschellic rays.
(4) Of Newtonic rays into Newtonic rays of less refrangibility.
(5) Of Newtonic rays into Herschellic rays.
(6) Of Herschellic rays into Herschellic rays of less refrangibility.
(7) Of Herschellic rays into Herschellic rays of greater refrangibility.
(8) Of Herschellic rays into Newtonic rays.
(9) Of Herschellic rays into Ritteric rays.
(10) Of Newtonic rays into Newtonic rays of greater refrangibility.
(11) Of Newtonic rays into Ritteric rays.
(12) Of Ritteric rays into Ritteric rays of greater refrangibility.

4. *Degradation and Elevation.*—Of the enumerated twelve species of transmutations, the phenomena of fluorescence and phosphorescence afford only instances under (2) and (4); for, as previously stated, these phenomena consist in an emission of light, or Newtonic rays, consequent upon the incidence of either Newtonic or Ritteric rays, subject to the law that in every case the refrangibility of the emitted rays is less than that of the incident. This law, which has particular reference to those cases where the incident rays are Newtonic ones, has been found to obtain without exception in all known examples both of fluorescence and of phosphorescence*; and, since it comes of itself

* See Phil. Trans. for 1852, p. 499; and *Ann. de Chim. et de Phys.* vol. lv. pp. 114 and 117 (1859). In a few instances the incident and emitted rays have been found to be of equal refrangibility.

true whenever the incident rays are Ritteric ones, the presumption has arisen that only such transmutations may actually occur in nature as involve a decrease of refrangibility in the emitted ray as compared with the incident. If such were the case, it is evident that the species of transmutations instanced above from under (1) to (6) would alone be possible, whilst the remainder would be impossible by the nature of things. Among the transmutations the possibility of which is thus more or less directly negatived, the species (8) and (10), forming in some sense the counterpart of those properly comprised under fluorescence and phosphorescence (namely of (2) and of (4)), it would undoubtedly be of the greatest importance to effect. A simple consideration of well-known facts will show that the transmutations (8) and (10) may probably be effected.

5. *Calcescence.*—The metals, by Professor Stokes, are classed among non-fluorescent substances*, and by M. E. Becquerel, to whom, as is well known, the most delicate observations on phosphorescence are due, among non-phosphorescent substances†—Professor Stokes having been unable to elicit Newtonic rays from metals by the means found efficient with fluorescent substances, and M. Becquerel having been unable to detect any persistency of luminosity in metals exposed to the treatment of the phosphoroscope. On the other hand, it is well known that metals may be rendered self-luminous or incandescent by contact with flames of high temperature, by electrical and other means; and, though no experiment appears to be on record affording clear evidence of the fact, it is impossible to doubt that the same effect might also be produced by solar radiation of sufficient intensity. If this latter assertion be founded on truth, it must be evident that all metals, in the wide sense of the word, are fluorescent, and most probably also phosphorescent; for of these phenomena no other definition can for the present be given than that of an emission by renovation of luminous rays on the part of substances irradiated from without, which would clearly be applicable to the case of any metal rendered incandescent by means of insolation. Or, if the definitions of fluorescence and phosphorescence be restricted so as to apply only to facts of the same order as hitherto discovered, namely, to emissions by renovation of luminous or Newtonic rays, on or after incidence of either Newtonic or Ritteric rays, even in that case it may be shown that the metals have a claim to be classed among fluorescent, and then most probably also among phosphorescent substances.

Every kind of radiation possesses, with respect to any given

* Phil. Trans. for 1852, p. 516—"Metals proved totally insensible."

† *Ann. de Chim. et de Phys.* vol. lvii. p. 45 (1859)—"Les métaux n'ont donné jusqu'ici aucun effet appréciable."

substance, a certain heating power, which depends (1) on the amplitude of the given ray, (2) on the absorptive power of the given substance for the given ray, and (3) in some unknown manner on the length of undulation of the given radiation. Any kind of radiation may hence be competent to raise any substance whatever to any required temperature by a suitable adjustment of the element of amplitude alone, provided the substance considered be not absolutely pervious to, or an absolute reflector of, the given radiation; more particularly must any species of Newtonic or Ritteric radiation be competent to raise any metal to the fixed temperature of incandescence, if the radiation have sufficient amplitude and be not of that quality which exceptionally may render it liable to absolute reflexion. The conclusions here stated are warranted by all our present knowledge regarding the nature of heat, temperature, and radiations. For, however small may appear the calorific effect of the more refrangible part of an ordinary solar spectrum—whether this be owing to the comparatively small amplitude of the radiations composing it (which we are unacquainted with), or to the peculiar nature of the undulations, which renders them less absorbable or otherwise unfit to produce as great heating effects as the less refrangible radiations, independently of the effects of dispersion—in no case where actual absorption takes place can this heating effect be absolutely nought*. But metals, by the teaching of experience, do absorb both Newtonic and also Ritteric rays†, the heating effect of which, by an increase or addition of amplitude, may hence be augmented to any wished-for degree, and more particularly be made to result in incandescence—theoretically, and saving practical difficulties.

The above considerations clearly demonstrate that, if no other reasons militated to the contrary than the apparent results arrived at by experimenters, it were right to class the metals among fluorescent and also among phosphorescent substances; and, in the next place, what is even more important, that, by means of metals at any rate—and probably by the very substances hitherto distinguished as fluorescent—the opposite transmutations of those which are ordinarily effected by fluorescent matter, and which were believed to be alone possible, might be realized. For it will be evident that incandescence, or an emission of Newtonic rays, which, as proved, might be engendered even by Rit-

* Compare the interesting experiments and remarks by Dr. Draper, in Phil. Mag. vol. i. pp. 93–95 (1851). [It is supposed of course that the absorption does not produce any chemical or other similar effects.]

† With regard to Newtonic rays, the colour of metals is a sufficient proof of their absorptive power. For proof of the absorptive power of metals with regard to Ritteric rays, see the observations by Prof. W. A. Miller, in Proc. Roy. Soc. No. 51. p. 163 (1862).

teric rays, will be still more easy to produce by means of Herschellic rays, to which, for some reason or other, a greater heating power is universally acknowledged to belong, as it will be possible also to produce by any given luminous or Newtonic radiation incandescence of such intensity that some of the Newtonic rays emitted should exceed in refrangibility the incident. In other words, through the production of incandescence by irradiation, or more particularly insolation, the transmutations previously denoted by (8) and (10) might be accomplished.

6. *Experiments.*—To test the exactness of the preceding inferences, the author, conjointly with Mr. G. Griffith, Deputy-Professor of Experimental Philosophy in the University of Oxford, has instituted the following experiments. At the focus of an ordinary concave glass mirror, measuring some 2′ across*, and irradiated by the sun, a piece of platinum-foil was exposed, attached to the bottom of an opake tube, so that the back side of the foil might be observed free from the interference of any extraneous light, such light only being seen as the foil itself might emit on becoming incandescent. On days of powerful sunshine the platinum became vividly incandescent, and, viewed by means of a pocket-spectroscope (which was inserted in the above-mentioned tube), a spectrum exhibiting all the visible or Newtonic rays might be observed. On placing, however, in the path of the reflected cone of rays, between the mirror and its focus, a sheet of monochromatic red glass (which, of all substances capable of absorbing the more refrangible part of the spectrum, allows the less refrangible part perhaps the freest access), the incandescence was found to be extinguished, or at least to become so faint as to be of doubtful visibility. It was needless to try, under these circumstances, whether the rays reflected by the mirror were capable of producing incandescence after having traversed a diaphragm allowing access only to Herschellic rays, and absorbing all the rest.

Having found the mirror by itself thus far inefficient, it was intended to resort to an expedient founded on the following considerations. Let the heating effect of the rays transmitted by the diaphragm of red glass, in the above experiment, be designated by α, and that of the rays absorbed by the same by β. It having been found, as mentioned before, that $(\alpha+\beta)$ was sufficient for the production of incandescence, let the experiment be made as first described—that is to say, the mirror be exposed to the sun, the platinum placed in its focus, and the red glass be interposed—but let the platinum besides be connected with a galvanic battery capable of replacing to it the heating effect β, lost by absorption in the diaphragm; then it appears

* [The principal focal distance was $2\frac{1}{2}'$.]

necessary that the platinum should become incandescent as heretofore, when irradiated by the whole cone of rays which is found concentrated at the focus of the mirror. An experiment of this nature (which supposes besides that β by itself is not competent to produce incandescence) would be open, however, to exception, in consequence of the dependence of electrical resistance on temperature; but it might be varied in the following manner, in which it would become entirely unexceptionable. The apparatus being all arranged as above, let an opake screen be at first interposed between the mirror and the platinum in its focus; but, on the other hand, let the platinum be rendered incandescent by the agency of the galvanic battery alone with which it is connected. Then, breaking this connexion, let the moment be seized at which the platinum, though still hot, ceases to shine, and let at the same instant the screen mentioned above be removed, so as to allow the rays reflected from the mirror to impinge upon the platinum, after having traversed the red diaphragm. These rays being of sufficient calefactory power (as found upon trial) to burn dry paper and pieces of wood, will presumably be competent to revive the incandescence of the platinum, if dexterously applied at the very moment of its extinction. The experiment being made either in the first way or in that just described, it would be necessary to observe whether the incandescence produced is visible if looked at through an eye-glass of such a nature as to be certain that all rays of equal or less refrangibility than those transmitted by the red glass are absorbed, whilst only such as are of higher refrangibility are permitted to reach the observer. Such a medium might be found in certain varieties of green glass, which absorb all the rays of the spectrum of lower refrangibility than the line D—supposing the so-called monochromatic red glass transmitted only these very rays, and none of greater refrangibility. This is generally supposed. On looking, however, at the sun with a pocket spectroscope through the very deepest red glass, it was easy to notice that, besides the rays mentioned, it transmitted, under the circumstances, rays of greater refrangibility, extending towards the blue region of the spectrum, but separated from the red and orange by a perfectly dark band; so that it became at least doubtful whether this and other red glass, on being employed for the above experiment in the way described, might not allow rays of higher refrangibility to impinge on the platinum than the green glass is capable of absorbing. The investigation of this point and other untoward circumstances did not allow of the experiments suggested to be performed in time to be communicated, except as a project, to the Association*.

* The Association have since placed at the disposal of the author and Mr. G. Griffith a grant of money to carry out the experiments suggested by him with a more powerful mirror than was employed in the above-described trials.

One interesting observation, made in the course of preparing for the preceding experiments, may be worthy of mention. The platinum being rendered incandescent in the focus of the mirror by insolation, a film of water of some 2″ thickness, contained between thin sheets of glass, was interposed, and no other diaphragm, when the luminosity of the platinum quite disappeared, just as upon the interposition of the red-glass diaphragm. By the experiments of Masson and Jamin*, the absorption exerted by glass and water of the thickness described upon the visible or Newtonic rays is extremely small, and according to Professor W. A. Miller† the same may be pronounced also with regard to the absorption of the Ritteric rays; but, on the other hand, it is well known that glass, and even more so water, very powerfully absorb the invisible Herschellic rays. It is evident, therefore, that the sudden disappearance of the incandescence of the platinum-foil upon the interposition of the above water-and-glass diaphragm is owing mainly to the abstraction of a great amount of Herschellic rays from the incident beam. Hence it is proved that in the first-described experiment, where all the three species of rays impinge upon the platinum, the Herschellic rays *contribute* to the production of incandescence—that is to say, of luminous or Newtonic rays—though this does not actually prove that Herschellic rays by themselves are capable of causing incandescence‡. It might, besides, be objected to the above inference, that the ceasing of incandescence, in the case described, might possibly be owing to the loss of luminous and other rays by reflexion at the several surfaces of the diaphragm which had been interposed. This objection, however, may be obviated by simply remarking that, upon employing a thinner film of water—the diaphragm being otherwise similarly constructed, and therefore involving the

* *Comptes Rendus*, vol. xxxi. p. 14 (1850).

† Proceedings of the Royal Society, No. 51.

‡ [A diaphragm of green glass acts in the same manner as the water diaphragm; that is to say, by interposing such glass between the mirror and the platinum at its focus, the production of incandescence is prevented, the incandescence making its appearance as soon as the diaphragm is omitted. It might be (and indeed it has been) said that in this case, as in that of the water, the Newtonic rays which are incident originate vibrations, corresponding to Newtonic rays, on the part of the platinum-particles, and that the function of the Herschellic rays which are added (upon the omission of the water diaphragm, for instance) is only to increase those vibrations and to render them perceptible. It is difficult to conceive, however, that an agent which is capable of increasing a certain mechanical effect should not be capable also of originating it; and I am therefore inclined to consider the fact adduced in the text, and the corresponding one relating to green glass, as really, though indirectly, proving the possibility of ray-transmutations implying an elevation instead of a degradation of the rays in the scale of refrangibilities.]

same losses of radiation by reflexion, but a smaller loss of Herschellic rays by absorption—the incandescence was found to continue (though of course its brilliancy was weakened) notwithstanding the interposition of the diaphragm.

7. *Transmutation in Comburescence.*—Another class of phenomena from which corroboration may be derived that transmutations involving an increase of refrangibility in the emitted beam as compared with the incident—and more especially the transmutation of Herschellic into Newtonic rays—are feasible, may yet be briefly adverted to. The glow of a platinum wire held in the flame of a Bunsen's gas-burner, of carbon particles in the candle-flame, or of lime in the oxyhydrogen-flame, and no less so the phenomena of coloration to which the introduction of substances capable of vaporization gives rise in alcohol or gas-flames, in the opinion of the author, constitute examples of ray-renovation or transmutation in *statu nascenti*, so to speak, of the rays. In all these instances the matter introduced into the various flames does not produce any new rays by chemical or other means, but acts simply as a renovating and transmuting agent on the rays emitted by the comburescent or ignited gases with which it is in contact*.

The above phenomena in so far are, if not identical, still extremely similar to those of ordinary ray-transmutation by fluorescence; but there is reason to believe that the transmutations which they evidence are the reverse of those effected by ordinary fluorescence. Perhaps the most trustworthy example for the deduction of such an inference is afforded by the case of the oxyhydrogen-flame and of lime-light. It is well known that the oxyhydrogen-flame by itself is but sparingly visible, and hence poor in Newtonic rays; it has been found to be little active photographically, and is hence rather deficient in Ritteric rays; yet on the introduction of lime into the flame, the well-known brilliant lime-light is emitted, consisting of a dazzling beam of Newtonic rays, which, to conclude from its powerful photographic effects, observed by Professor W. A. Miller†, is probably accompanied by an intense beam of Ritteric rays. As the oxyhydrogen-flame, from its great powers of calefaction, must necessarily emit Herschellic rays abundantly in its natural state, it admits of little doubt that the Newtonic and Ritteric rays engendered by the introduction of lime into the flame arise from a

* [It may be interesting to learn what was the opinion of Melloni regarding the origin of lime-light, &c. He says (*La Thermochr.* p. 94):—"Ce phénomène tient évidemment à une action de masse. Les gaz sont des corps très légers; s'ils peuvent réussir à communiquer leur état d'incandescence aux corps doués d'une plus grande densité, la quantité de lumière devra nécessairement augmenter."]

† Chemical News for March 21, 1863.

transmutation of Herschellic rays in the very act of emission. By means of a couple of conjugate mirrors, having in one of the foci an oxyhydrogen-flame, and in the other a piece of lime, the phenomenon of the transmutation of Herschellic into higher rays, thus spontaneously occurring in the production of the common lime-light, might be made to assume the ordinary form of fluorescent phenomena.

8. *Conclusion.*—In the original communication a third class of experiments promising to effect a conversion of Herschellic rays into Newtonic was adduced, which, from its being almost entirely conjectural, shall be here passed over*. There were also several distinctions mentioned, which most probably will divide ordinary fluorescent phenomena from those to which attention has been directed in this paper. To distinguish these latter phenomena from the former, the term *Calcescence* was suggested to the author, from *calcium*, the name of the characteristic chemical constituent of lime, whose action on the oxyhydrogen-flame suggested the preceding speculations and experimental attempts; and this term might be applied to all phenomena involving an emission of Newtonic rays by the transmutation of incident Herschellic rays, or generally an emission of rays of increased refrangibility as compared with the generative incident.

[9. *Addenda.*—(i) The experiments first tried in the summer of 1863, and briefly described in principle in the preceding pages, were resumed with the aid of more efficient apparatus in the spring of the present year. The mirror employed was one of glass, quicksilvered on the back, of 3′ aperture and $3\frac{1}{2}$′ focus. It was supported at the sides by means of two screws passing through two wooden uprights, properly connected to form a stand. At its vertex, or highest point, a socket was fixed to the mirror, in which a flat bar of iron, possessing the form of an elongated S, was made to rest and held firm by binding screws. The free end of this bar reached somewhat further from the mirror than the principal focal distance, whilst its projection on the plane of the mirror fell a little short of the centre of the mirror. To this end of the bar, which was rounded and perforated, a brass ring or clip was attached, by means of a moveable little brass rod held firm by a binding screw. The ring was placed as nearly as possible concentrical with the mirror, and admitted of a wooden tube, which, when placed so that its nearest end was exactly at the distance of the principal focus from the mirror, was held in its position by means of a screw and nut attached to the brass ring or clip adverted to. At the end nearest to the

* [The whole of the paper adverted to has appeared since in the Reports of the British Association for 1863.]

mirror, the wooden tube referred to, the diameter of which was about 2″, was closed by means of two thin plates of slate cut out at the centre, and between which the platinum-foil intended to be rendered incandescent was placed. These plates of slate were designed to close up the tube and render it light-tight, holding at the same time the platinum in its place, and preventing as much as possible caloric conduction*; they rested on a slight circle or shoulder on the interior of the tube, and were prevented from slipping or dropping by means of an ivory ring screwed on the exterior of the tube. This ivory ring bore, at two points diametrically opposite, two angular pieces of brass of this form ⌐—, the shorter and vertical limbs of which had binding screws attached, to make connexion with wires leading to a galvanic battery, whilst the longer and horizontal limbs, near their free extremities, bore each a vertical screw with a flat base made of platinum. The bases of these screws could be screwed down, and made to rest upon the exposed face of the platinum-foil—the upper slate being cut out nearly elliptically, whilst the lower slate was cut out circularly, the diameter of the circle being about equal to the short axis of the ellipse. Thus, when the ivory ring was connected with a galvanic battery, the current passed from one of the angular pieces already described, through the platinum-foil (of about 1″ breadth, and 1 to 1½″ length), to the other angular piece†.

The tube thus partially described was about 1′ long, and could be connected with another and similar tube of rather greater length than the semidiameter of the mirror—the two tubes, when connected, forming an angle of 90°. At the interior of this angle a piece of looking-glass was placed, making angles of 45° with each of the two axes of the tube‡. By this means,

* To some extent the plates of course conduct away the heat of the platinum. This circumstance, however, is not without advantage, at least in the case of such experiments as before mentioned, in which it was desired to apply solar radiation subsequent to galvanic heating. In these experiments it is desirable that the platinum should remain hot, as long as possible, after the galvanic heating has ceased—a result which was materially aided by the contact of the platinum with a substance such as slate, which, having been itself heated at first by the galvanic current, afterwards retained its heat much longer than the platinum, and partially conveyed it back to the platinum.

† The thicknesses of the foils varied from $\frac{1}{500}$″ to $\frac{1}{6005}$″; some of them were of silver platinized. The screws which rested on the foils were sufficiently apart not to interfere with the cone of rays incident on the foils.

‡ The whole of the tube consisted of three parts—one angular, and the two others straight. By joining the latter two parts together, of which one supported the foil and the other the ocular diaphragm, the tube could be used for direct vision, without the intervention of reflexion. The angular or bent piece was sheathed on the outside in brass, and the angle itself was cut off by a circular section, leaving a hole into which was fitted a cork, on

an observer placed at the end of the tube, which being parallel to the mirror reached just a little beyond it, might see the inner surface of the platinum-foil without casting a shadow on the mirror, although the mirror faced the sun in the most advantageous position, viz. so as to concentrate the solar rays at the principal focus.

Next after the mirror and the tube, the most important piece of apparatus consisted in the diaphragm or diaphragms by which the solar rays were to be sifted. To avoid every objection, it was intended to have these diaphragms a little larger than the mirror itself, and to connect each as it might be employed laterally and all round with the mirror, so as to allow of the incidence of no rays upon the platinum except such as had previously passed through the diaphragm. Diaphragms of this size, however, and of the qualities required, were not to be procured; and it was thus necessary to employ much smaller ones, which were placed between the mirror and the platinum in frames supported by stands. In this manner some of the rays incident had to pass twice through the diaphragm, first on incidence, and next after reflexion. To obviate the objection arising from the incidence of side-light upon the platinum, it was proposed to employ a truncated tin cone, the base of which was formed by the diaphragm itself, whilst the apex gave admittance to the tube supporting the platinum*. In this manner, no rays could reach the platinum but such as had previously passed through the diaphragm; but, on the other hand, the cone itself would have cast a shadow on the mirror, if the latter was placed in its most favourable position.

The diaphragms to be employed were of three kinds—(1) red glass, (2) black glass, and (3) iodine dissolved in bisulphide of carbon contained between thin sheets of glass. As regards red glass, we succeeded at last in procuring from Birmingham panes of glass about 1′ square, which, tested by the pocket-spectroscope, cut off all rays of greater refrangibility than the line D. When combined with green glass of the deepest tint, red glass of this description made the sun appear red; which showed that the green glass was slightly transparent to red rays, although when tested by the pocket-spectroscope it cut off all rays of less refrangibility than the line D. Hence it is evident that the analysis by the spectroscope in the case of the red glass also

which the piece of looking-glass was mounted. When the looking-glass was properly adjusted, the hole was stopped up light-tight by means of a brass cap, which fitted on the cylindrical borders of the hole.

* The cone was pierced in two places, to admit of the wires of the battery, properly insulated from the substance of the cone.

could not be wholly relied upon, as regards its absolute imperviousness to rays of higher refrangibility than the line D*. As regards black glass, Melloni has published the following numbers, showing its relative permeability to rays from different sources, the incident rays being reckoned as 100† :—

	Thickness.	Alcohol-flame.	Incandescent platinum.	Oil-flame.
	millim.			
Colourless glass ...	0·88	41·2	52·8	70·6
Black glass.....	0·62	52·6	42·8	37·9
Black glass.....	1·84	29·9	27·1	25·3

M. A. Matthiessen of Altona also has published observations, made on the solar spectrum, which prove the permeability of opake black glass (manufactured at Choisy-le-Roi) to the invisible Herschellic rays‡. I consequently tried to procure such black glass both from London and Paris, but only towards the end of last summer I succeeded in getting a specimen from Paris, through Mr. Becker; the absorbing and transmitting powers I have not yet been able to test. Finally, as regards the solution of iodine in bisulphide of carbon, my attention was directed to it by a passage in the work on Heat, by Prof. Tyndall, who has recently published further experiments on the subject. Considering, however, that the iodine solution must always be placed between glass sheets, and that the transparencies of white and black glass for Herschellic rays seem to be nearly equal, whilst extremely thin sheets of black glass seem capable of excluding the light of the most powerful sources§, perhaps black glass, upon the whole, will appear preferable as a diaphragm to the solution of iodine.

Another species of diaphragm required, and to be used as an

* However, in no case is it possible to be sure that a given radiation is totally absent; all that it is possible to aver is that it is imperceptible. For this reason it is perhaps equally decisive to prove, in such experiments as those that were to be instituted by means of the above red glass, that the radiation emitted is of greater intensity than the incident, as to prove that the radiation emitted is perceptible, whilst in the incident beam the same radiation is apparently wanting—that is to say, possibly only imperceptible. It would be necessary, however, in the former case not merely to consider the momentary intensity, but to compare the energy represented by the incident rays with the whole of the energy represented by the corresponding emitted rays, *as long as the emission lasts.*

† *La Thermochrôse*, p. 178. Compare also the coresponding data in pp. 227, 265, 295, 298, 309, and 310.

‡ *Comptes Rendus*, vol. xvi. p. 763 (1843).

§ According to Melloni, black glass of the kind employed in the experiments above quoted ("such as opticians employ for the construction of polarizing mirrors") is competent to intercept the light of "the most brilliant sunshine" in thicknesses as small as 0·596 millim. (*La Thermochrose*, pp. 289 and 310.)

ocular diaphragm when the red glass was used as an objective diaphragm, was deep-green glass. Pieces of such glass were used for insertion within the angular tube near the end where the eye was placed, and were made moveable from without; so that the incandescence of the platinum, when produced, could be viewed either freely or through the ocular diaphragm.

It will be seen that our arrangements were pretty well considered. Unfortunately, when our preparations were nearly matured, my collaborator, Prof. Griffith, as it appeared, was not able to give to the subject so much of his time and attention as it required, and towards August this year the experiments were consequently allowed to drop unfinished.

(ii) I have stated in a former Number of the Philosophical Magazine my probable inability to pursue the subject-matter of this paper any further, at least experimentally*. Meanwhile I will take this occasion to make some remarks concerning experiments on artificial sources of rays.

Our first trials at Oxford were made with a couple of brass reflectors and the flame of the oxyhydrogen jet, in the manner before described; but, no doubt in a great measure from the imperfect condition of the reflectors, the trials were not successful. It is evident that, instead of a couple of reflectors, a single reflector might be made to render practically the same service, the flame and the object to be rendered incandescent being respectively placed at any two conjugate foci of the reflector. The oxyhydrogen jet has the advantage in these experiments of being comparatively deficient in Newtonic and Ritteric rays, which might hence be easily eliminated altogether without much detriment to the Herschellic rays. On the other hand, gases are known to be but indifferent radiators as compared with solid bodies, as was shown long ago by Melloni†. Hence it might be preferable perhaps to use lime-light for the purposes here in question, instead of the pure oxyhydrogen-flame, as possibly the Herschellic radiation of lime-light remaining after the Newtonic and Ritteric rays have been eliminated by proper absorbents might surpass in intensity the similar radiation emanating from the pure oxyhydrogen-flame.

Lampblack having been shown by Melloni to be a better absorbent of nearly all kinds of radiations than chalk or lime‡, incandescent coal is presumably a more prolific source of Herschellic rays, the temperature being the same, than incandescent lime. For this reason, coal or coke, rendered incandescent either in the

* Since this paper was put in type, I have been led to hope, by authorized members connected with a great scientific society, that means will be found to set my experiments on foot again.

† *La Thermochrose*, p. 94. ‡ Ibid. p. 96.

oxyhydrogen-jet* or by electricity might be preferable to lime-light for the experiments here spoken of. Coke made incandescent by electricity, or the so-called electric light, presents, with regard to the other sources of rays just mentioned, the following further advantages:—(1) According to Prof. Stokes the spectrum of electric light is much more extended in the direction of the Ritteric rays than the solar spectrum†. Now, according to Prof. Draper's observations, the higher the *maximum* refrangibility of the rays emitted by any incandescent solid substance, the higher is its temperature, and the greater also is the intensity of the rays of lower refrangibility‡. Hence it might seem probable that the joint intensity of the Herschellic radiations emitted by the electric light are greater than those of the similar radiations emanating from equal parts of the sun§. (2) The temperature of the electric light, or of the coal points by means of which it is produced, is considered to be higher than that of the oxyhydrogen-flame, and consequently also higher that that of lime-light‖. I had frequently desired to test the transmitting powers of the various diaphragms before mentioned by the aid of the electric lamp, but I had no opportunity of carrying out my design¶.

(iii) Other subjects of experiment, which suggested themselves to me from their relation to the principal subject-matter of this paper, I shall point out here very briefly.

(1) One of the first questions which arose in my mind in connexion with the explanation of the phenomena of lime-light, &c., was whether in consequence of the ray-transmutations therein taking place the total emission of rays was altered. I later found that Melloni had long ago shown such to be the case in instances where the transmuting agent is a solid. M. Magnus, recently, has shown that the same result obtains also in

* According to M. E. Becquerel, however, "coke placed in the flame of the oxyhydrogen blowpipe emits a light but little different from that of a piece of magnesia or lime" (*Ann. de Chim. et de Phys.* 1863, vol. lxviii. p. 139.

† Proc. Roy. Inst. vol. i. p. 264 (1853).

‡ Phil. Mag. vol. xxx. p. 345 (1847).

§ Compare, however, the experiments of MM. Fizeau and Foucault in *Comptes Rendus*, vol. xviii. p. 746 (1844); and of MM. De la Provostaye and Desains, ibid. vol. xxxi. p. 515 (1850).

‖ See M. E. Becquerel, *loc. cit.* pp. 137 & 139.

¶ I have reason to know that, within the last month or so, experiments have been made at the Royal Institution on calcescence, by means of the electric lamp, holding out great promise of success. It is stated that ignited magnesium wire, which emits a light of great brilliancy, is rather deficient in radiations of comparatively low refrangibility; otherwise a magnesium-wire lamp would offer great advantages as to convenience over any of the other sources of rays above mentioned.

instances where the transmuting agent is a vapour*. The increase of radiation exhibited in these experiments, at first sight, appears equivalent to a creation of energy; but, upon consideration, it is easy to see that such need by no means be the case. Some direct experiments might perhaps be made on this point.

(2) The experiments before described, in which the platinum was to be heated by electricity previously to the incidence of solar rays, together with a remark mentioned below, led me to propose to myself an investigation of the absorptive powers of solids for rays as dependent on temperature†.

(3) In my original paper, since published in the Reports of the British Association for 1863, I suggested an explanation of the phenomena of ray-renovation, according to which the rays emitted in the act of renovation are to be considered as resulting from a species of interference between the incident or absorbed rays and the rays spontaneously emitted by the renovating substance. In order to test this view practically, I intended to try the following experiment. Let a fluorescent substance be placed in front of a thermo-multiplier, and between the multiplier and that substance let an absorbing medium be placed, capable of eliminating Newtonic rays of that very description which the substance in question gives out in the act of fluorescence. The caloric equilibrium being established whilst the substance does not fluoresce, it is evident that, as soon as the substance begins to fluoresce, the caloric equilibrium will be disturbed—supposing that in the act of fluorescence some of the rays originally emitted by the fluorescent matter get, as it were, consumed (as they must in a case of interference), and supposing also that no merely reflected rays are allowed to pass from the fluorescent substance through the absorbing medium on to the pile.

(4) Finally, besides some other points not ripe enough to be here mentioned, I purposed to investigate the Herschellic spectra of the principal incandescent gases, that have recently been examined with regard to their Newtonic spectra. I also intended to endeavour to procure thermographic impressions of Herschellic spectra of all kinds.]

London, December 1864.

Corrigenda.

Vol. xxviii. p. 475, line 7, *for* § 207, i. *read* art. 2071.
— p. 477, line 23, *for* centripetal *read* centrifugal.

* Pogg. *Ann.* vol. cxxi. p. 510 (1864).

† In Phil. Trans. vol. xxx. p. 977 (1719), it is stated, regarding a certain metallic mirror, that "the glass [*i. e.* the mirror] growing hot burned with much less force." This, I thought, might be a proof of the dependence of absorption on temperature, although it is possible to assume that the fact adverted to may have arisen from some other circumstances.

VIII. *On the History of Negative Fluorescence.* By JOHN TYNDALL, *F.R.S. &c.**

A COMMUNICATION from the pen of Dr. Akin, published in the last Number of the Philosophical Magazine, will, I trust, be my excuse for giving the following brief sketch of my relation to the question of "negative fluorescence"—a term which may be provisionally taken to express the changing of the refrangibility of the invisible ultra-red rays of the spectrum so as to render them visible†.

In the month of June 1852, a scientific gentleman, eminent though young, showed me, in his lodgings in Duke Street, Piccadilly, the fluorescence of an infusion of horsechestnut bark, and explained to me the nature of the discovery of Prof. Stokes.

In the month of September of the same year, I heard Prof. Stokes lecture on this subject before the Belfast Meeting of the British Association. At that Meeting, moreover, a term was employed by the gentleman who had first enlightened me on the subject of fluorescence, which I have never since forgotten. He said that the light was always *degraded* (meaning thereby that its refrangibility was always *lowered*) when fluorescence was exhibited.

This lowering of the refrangibility was one of the most prominent characteristics of the phenomena brought to light by Prof. Stokes, and it inevitably provoked the opposite question. In common, I believe, with many of those who had heard the experiments described, I soon afterwards inquired whether it was not possible to *elevate* refrangibility, and thus to render the ultra-red rays of the spectrum visible, as Prof. Stokes had rendered the ultra-violet ones.

I believe I am right in saying that though, owing to the total absence of facts, nothing was published on the subject, the question of a change of refrangibility, in an upward direction, was not an uncommon topic of conversation among scientific men. In the year 1859, moreover, the writer already quoted, and who is also referred to by Dr. Akin, wrote as follows:—"The thought occurs

* Communicated by the Author.

† This term was introduced by M. Emsmann in 1861, in the following words:—"Starting from the fact that, in the phenomena of fluorescence hitherto observed, a lowering of the refrangibility or an augmentation of the wave-length occurs—a law which can by no means be regarded as firmly established—we cannot deny all justification to the opinion that a kind of fluorescence must also exist, the essence of which would consist in an elevation of the refrangibility or a diminution of the wave-length. The former kind of fluorescence would be that of the chemical rays, which I would propose to call *positive fluorescence*; the latter that of the calorific rays, which I would call *negative fluorescence*" (Pogg. *Ann.* vol. cxiv. p. 652). In 1863 Dr. Akin introduced the term *calescence* to express the same thing.

involuntarily, that phenomena analogous to those of fluorescence may be discovered, the explanation of which would have to be referred, not to a lowering, but a raising of the refrangibility. Such would be the case if the ultra-red rays of the spectrum could be rendered visible, as the ultra-violet ones have been"*.

During the last six years, or since the commencement of my researches upon radiant heat, this subject has been frequently in my thoughts; and I helped myself, whenever opportunity offered, to form conceptions of the physical processes involved in negative fluorescence, by observations upon waves of water. But other questions, of a more pressing character, compelled me to postpone the definite experimental examination of this one.

Early in the autumn of 1863 I imagined that I had succeeded in proving one of our commonest experiments to be an illustration of a change of refrangibility, in an upward direction. Having, as I conceived, shown the oscillating periods of the molecules in a hydrogen-flame to be ultra-red, I inferred, at once, that the light emitted by a platinum wire plunged into the flame could only be produced by a change of period. It was not, however, a case of real negative fluorescence, for there was no conversion of invisible *rays* into visible ones. The change of period occurred, as I then phrased it, "before the heat had assumed the radiant form."

Later on in the year I met, at a Philosophical Club dinner, a gentleman whose own experiments had given him the right to speak with authority in these matters, and I took my place beside him for the express purpose of learning his matured opinion as to the possibility of converting long waves into short ones. I mentioned to him the observations which I had been making from time to time on sea-waves in the Isle of Wight, and I then first learned that a gentleman named Akin was trying experiments on this subject.

The remarks made on this occasion were restricted to the conversion of *radiant heat* of slow period into heat of more rapid period. What I then conceived to be *my* illustration of conversion (namely, the change of the slow periods of a hydrogen-flame into the rapid periods of a platinum wire plunged into the flame) was not referred to. About this time, however, the subject was one of those on which I freely conversed with my more intimate scientific friends. To Dr. Debus, for example, I expounded, early in December 1863, the whole question from beginning to end.

On the 23rd of January, a clever and interesting article, entitled "Calcescence," was published in the 'Saturday Review.' The author, Dr. Akin, first proposed an entirely new termi-

* Marbach's *Physikal. Lexicon*, vol. vi. (1859) p. 1081.

nology for the phenomena of radiation. The changes of refrangibility possible to light and radiant heat were afterwards discussed, and an experiment was described, by which the author *proposed* to convert the ultra-red rays of the spectrum into visible ones.

In that article there was no mention of the form of conversion which I thought had been established by myself; and having at that time occasion to write to the editor of the 'Saturday Review,' I mentioned, in my note to him, my having already accomplished that which Dr. Akin proposed to accomplish. I informed him that my solution was already some months old, and was now on the verge of publication. I requested him, moreover, if he thought fit, to show my note to Dr. Akin.

These facts are distinctly in the recollection of the Editor of the 'Saturday Review,' who is perfectly willing to corroborate what I have here stated.

The supposed change of period exhibited by a platinum wire in a hydrogen-flame, constituted a brief episode in my memoir on "Molecular Physics," read before the Royal Society on the 17th of March 1864. Up to that date I had not the slightest notion that Dr. Akin had ever alluded to the subject. During the exposition of my paper I referred pointedly to Prof. Stokes, with the view of eliciting his opinion as to the reality of the conversion. That opinion, clearly expressed and ably sustained, was not favourable to the conclusion at which I had arrived. Prof. Stokes reasoned thus:—"When oxygen and hydrogen clash together to form aqueous vapour, vibrations of the respective atoms, in short periods, result; but these vibrations of the atomic constituents are soon imparted to the molecules of the compound formed by their union. Aqueous vapour constitutes, in fact, the 'ashes' of a hydrogen-flame; on these ashes the rapid vibrations of the single atoms are expended, thus producing heat of *lower* refrangibility. When a platinum wire is plunged into a hydrogen-flame, it simply takes up the vibrations which, in its absence, are imparted to the ashes; hence the possibility that, in its case also, we have a lowering, instead of a raising of the refrangibility." In reply to this striking argument, for which I was quite unprepared, I urged that the platinum might be brought to incandescence at some distance *above* the tip of the hydrogen-flame, where the vibrations of the atoms might fairly be supposed to have already expended themselves upon the vapour-molecules. Prof. Stokes, however, retained his opinion that the experiment was altogether inconclusive as to the real point in question.

After the reading of my paper, I went to the Isle of Wight, and, while there, an abstract of the paper was published in the

'Reader' newspaper. Immediately afterwards a letter, addressed to me by Dr. Akin, was forwarded to me, in which he drew my attention to what he had previously written, and expressed a desire to see me. I replied to this letter with all goodwill, and my answer, I have reason to believe, is still in the hands of Dr. Akin. After my return to town he called upon me, and among other things directed my attention to a report in the 'Reader' of the 26th of September 1863. In this report I found the following passages:—"The glow of a platinum wire in a Bunsen's gas-burner, of carbon particles in a candle-flame, and of lime in the oxyhydrogen-flame, and no less so the phenomena of coloration to which the introduction of substances capable of vaporization gives rise in ordinary gas-flames, in the opinion of the author, constitute examples of ray-renovation or transmutation *in statu nascenti* (so to speak) of the rays;" and again, with reference to the oxyhydrogen-flame, "it admits of little doubt that the Newtonic [visible] and Ritteric [ultra-violet] rays engendered by the introduction of lime into the flame, arise from a transmutation of Herschellic [ultra-red] rays in the very act of transmission."

On being made acquainted with these expressions, I did not halt to criticise the grounds of Dr. Akin's "opinion," but I at once wrote the following note, which appeared in the next Number of the 'Reader':—

"Athenæum Club, April 9, 1864.

"Through the kindness of its author, I have this day become acquainted with Dr. Akin's communication to the 'Reader' of the 26th of December 1863. From conversation with Prof. Stokes, I had obtained a general notion of the experiments in which Dr. Akin has been for some time engaged, but I now learn that on one of the points touched upon in the report of my last investigation—the proposed explanation, namely, of the glowing of a platinum wire in a hydrogen-flame—he has anticipated me by several months.

"John Tyndall."

I afterwards learned that Dr. Akin did not wait for me to set matters right, but had promptly put in his own claim on the 2nd of April.

I have thus shown how the idea of negative fluorescence entered my mind; I have explained the nature of my mistake in supposing that I was the first to recognize, in the heating of a platinum wire in a hydrogen-flame, a change of period, and I have described the reparation made when Dr. Akin's relation to the subject became known to me. It never occurred to me to criticise the scientific method by which Dr. Akin arrived at his conclusions. I believed them to be correct, and

freely conceded to his insight what I deemed wanting in his data. But now that he talks of my reasoning being the same as his, save that it exhibits the defect of a missing link, let me inquire what is his reasoning, and what is mine? As far as I can see, he does not reason at all. He founds his "opinion," that the radiation from a hydrogen-flame is ultra-red, on the defect of brightness which the flame exhibits. Now the flame of hydrogen, and other gaseous flames, have been over and over again compared, by chemists, with flames yielding *solid* products of combustion, the defective brightness, in those cases where solid matter was absent, being either referred to the extremely *dilute* character of the radiation, or to the fact that intense combustion generated rays of a refrangibility too high for the purposes of vision. What has Dr. Akin done to prove this notion incorrect? Nothing. He never, to my knowledge, made a single experiment on the radiation of a hydrogen-flame. Again, he speaks of the probable paucity of rays of high refrangibility in a hydrogen-flame, as one ground of his conclusions. On this point let us hear Professor Stokes:—"It appears that the feeble flame of alcohol is extremely brilliant with regard to invisible rays of very high refrangibility. *The flame of hydrogen appears to abound in invisible rays of still higher refrangibility*" (Phil. Mag. S. 4. vol. iv. p. 392). And again—"The effect of various flames and other sources of light on sulphate of quinine, and on similar media, indicates the richness or poverty of these sources with respect to the highly refrangible invisible rays. *Thus the flames of alcohol, hydrogen, &c. were found to be very rich in invisible rays*" (Proceedings of the Royal Institution, vol. i. p. 264). I have italicized sentences which are diametrically opposed to the assumed "poverty of rays of high refrangibility," on which Dr. Akin, in part, founds his opinion.

My "reasoning," such as it is, is contained in the December Number of the Philosophical Magazine, p. 457, and at pp. 524 to 526 of the December Supplement. I start from the principle that, from the transparency or opacity of any homogeneous medium to the radiation from any source, we may infer the *discord* or the *accord* of the vibrating-periods of the source and medium. I illustrate this principle by a series of experiments on carbonic acid, aqueous vapour, and water. First proving the periods of water to be in accord with those of the ultra-red waves, I give reasons for concluding that the periods of a hydrogen-flame are in accord with those of water. Hence the inference that the periods of a hydrogen-flame are ultra-red, and that, when a platinum wire is raised to whiteness by the heat of such a flame, we must have a change of period.

Now these experiments, and the reasoning founded on them,

may or may not be "rigid;" but, at all events, they are *not* the experiments and the reasoning of Dr. Akin. The intelligent reader will now form his own estimate of the gravity of my offence when I ventured to put myself forward as an humble corroborator, by reasoning of my own, of views which I stated to have been previously enunciated by another.

With regard to my reading habits, I would simply say that it is hardly becoming on the part of a gentleman in his position to lay down the law, on this head, for the hardworking experimenter. Dr. Akin has, thus far, done little but read and write. I do not object to this, but I do object to his compelling me to adopt his habits. As a matter of principle, I reduce my reading to a minimum; and high as my opinion is of the functions of the British Association, I do not pay strict attention to the newspaper reports of its proceedings. This accounts for the fact that, until Dr. Akin drew my attention to it, I was not aware of the existence of the passage which he has cited from the 'Athenæum.' That passage, however, imposes no new duty upon me; I have already explicitly recognized the priority of Dr. Akin with regard to the point in question.

But the reader of the "Note on Ray-transmutation" will have already surmised that the seat of its author's discontent lies far deeper than this question of a platinum wire and a hydrogen-flame. Let us now inquire into his relationship to the real problem, which may be broadly stated thus:—

To raise the refrangibility of invisible rays of long period, so as to convert them into visible rays.

This problem would be solved by raising an incombustible body to a state of incandescence, by perfectly invisible rays of low refrangibility. The consideration of the problem at once limits us to those obscure rays of great intensity which are known to be emitted from highly luminous sources; for it would never occur to any one practically acquainted with the subject, to attempt to produce incandescence by rays emanating from an obscure source. The field of experiment is thus narrowed at the outset, and I am only acquainted with two sources which offer any prospect of success. These are the sun and the electric light. The obscure radiation of the sun was established in the year 1800 by Sir William Herschel, who then proved by far the greater portion of the sun's thermal power to be due to invisible rays of low refrangibility. I am not aware whether anybody but myself has worked at the invisible radiation of the electric light, but I may say that this subject has occupied me, at frequent intervals, during the last ten years.

Limited thus to rays emitted by luminous sources, the two obvious conditions of experiment are the suitable concentration

of the rays, and the removal of the luminous portion of the radiation. The second of these conditions constitutes the main difficulty; and we shall presently see whether the mode of surmounting it proposed by Dr. Akin entitles him to compare his performance with that of the mathematicians "who first conjectured the existence of Neptune," and so on. He described before the British Association, assembled at Newcastle in 1863, three experiments. In the first of them he proposed to employ two conjugate mirrors; at the focus of one of which he places a piece of chalk or lime, and at the focus of the other an oxyhydrogen-flame. He proposed to cut off "by absorbents" such visible and ultra-violet rays as the flame emits. "Then," he says, "if the mirrors are of sufficient size to render the temperature of the distant focus approximately equal to that of the flame itself, there is every reason to believe that the lime therein contained will shine out."

Six years of hard labour at these phenomena of radiation have rendered such proposals rather amusing to me. The "temperature at the distant focus" must, of course, be derived from the *rays* emitted by the oxyhydrogen-flame, which are reflected in a parallel beam by one mirror and concentrated by the other. It never occurs to Dr. Akin to inquire what fraction of the heat of an oxyhydrogen-flame is disposed of *by radiation*. He does not at the present moment know whether the tenth, the hundredth, the thousandth, or the ten thousandth part of the heat of the flame is thus disposed of. What he *imagines* is plain enough—namely, that, save some slight losses in his "absorbent" and at the surfaces of his mirrors, the *whole heat of the flame* is radiated against one mirror and condensed by the other. He entirely forgets that a flame may be intensely hot, and its radiation extremely feeble, and that this, in an eminent degree, is the case with the oxyhydrogen-flame. It is not the practical difficulties, which Dr. Akin himself discerns, that I am now speaking of; it is the radical vice of the conception that a purely gaseous flame, placed in the focus of a mirror, however large, could possibly generate a temperature "approximately equal to that of the flame itself," in the focus of another mirror.

In his second, and only rational experiment, Dr. Akin proposes to concentrate the sun's rays by a concave mirror, and to withdraw from the focus the luminous portion of the radiation. But then comes the question, How is this to be effected? Dr. Akin replies, "by proper absorbents." This, as far as I know, constitutes his entire answer to the question. In all that he has written upon the subject I have not been able to find a hint of what the proper absorbent is to be.

As a proposed experimental demonstration of a point which

can only be decided by experiment, Dr. Akin's third proposition is, if possible, more hopelessly absurd than his first.

These are the "ideas" of Dr. Akin, which I would have gladly let him enjoy, had he permitted me to do so. The fact is, that although he can fairly claim the credit of first proposing, in public, a definite series of experiments, with a view to the solution of this question, his Note compels me to state that other and more capable investigators, than he has proved himself to be, abstained from proposing such experiments, simply because they saw more clearly than he did the difficulties involved in the practical treatment of the problem. This, I imagine, sufficiently accounts for a fact which appears to have taken him by surprise, namely, that notwithstanding his having proposed so "simple" a plan, "he had vainly endeavoured, for nearly a year, to procure for it a practical trial." The wished-for opportunity at length came, and the practical trial was made. Concentrating the solar rays by a concave mirror 18 inches in diameter, Dr. Akin found that, when no absorbent was introduced, a piece of platinum-foil, held in the focus of the mirror, was rendered incandescent. But when he introduced, between the mirror and the focus, a piece of monochromatic red glass, "which of all substances, capable of absorbing the more refrangible part of the spectrum, allows the less refrangible part the freest access, the incandescence was found to be extinguished, or at least to become so faint as to be of doubtful visibility" (Reader, September 26, 1863, p. 349). It is needless to remark that, even had this experiment succeeded, the question would have still remained unsolved; for a sheet of glass, which permits the most powerful rays of the visible spectrum to pass through it, could not be called a "proper absorbent." He was afterwards joined by the excellent Assistant Secretary of the British Association, and in his article in the 'Saturday Review' expressed the hope, "that in the course of next summer he will bring his experiments to a successful termination." The summer came, and a better one for his purpose rarely favoured England —strong sunshine, and plenty of it; and what is the result? Failure, but no abatement of pretension. "I have no doubt," he says, "that, with the means at his command and his experimental proficiency, Prof. Tyndall will now realize and 'publish' a discovery which I have assigned the methods for accomplishing, and which I should have probably effected myself, I may say, years ago, had I been seconded as I had hoped, either by persons or by circumstances." Dr. Akin knows perfectly well how safe it is to boast "*now*" that he could have made the discovery referred to. He has the best possible reason for having "no doubt," viz. the sight of his eyes.

As already stated, the obscure radiation from the electric light has occupied my attention, more or less, during the last ten years. In 1854 I sought by "proper absorbents" to separate the luminous from the non-luminous radiation of this source. In 1858 I again tried to do so; and it was only a few days ago that the last remnant of the black glass, prepared for me for this express purpose by the late Mr. Darker, went to pieces in the condensed beam of my electric lamp. It is not my habit, nor do I think it a commendable habit, to sit down and propose experiments which may, or may not, be capable of realization. At all events if this be done at all, it ought to be done in a magnanimous spirit. The true experimenter knows how frequently the most promising ideas are shattered when he tries to realize them. He is forced to be an iconoclast from day to day, breaking down the idols on which his hopes were fixed only to be frustrated. Such a man is not likely to sit down and write out experiments at his leisure, with a view to mounting the high horse of "Neptune," and claiming the credit, should anything similar be afterwards executed. My own experience of an experimenter's difficulties—difficulties which apparently had never dawned upon Dr. Akin when he made his experiments on paper—are referred to in my book on Heat, page 333. This same book, which is an account of lectures given at the Royal Institution a year and a half before the scientific advent of Dr. Akin, shows me in the act of employing such "proper absorbents" as were then known. At page 307 I describe experiments in which, by means of a plate of rock-salt, coated with the smoke of a lamp, I cut off the luminous portion of the beam from the electric light. I mark, by a rod, the focus of the invisible transmitted rays, and, bringing my thermo-electric pile into this focus, cause the heavy needles of my coarse galvanometer to dash violently against their stops. A similar experiment, with black glass, is described at page 308. But, though this substance transmitted obscure heat much more copiously than the lampblack, the "absorbents" which first filled me with hope, if not with confidence, are mentioned at page 351. These absorbents are bromine, and a solution of iodine—substances suggested primarily by my own researches on the deportment of elementary bodies towards radiant heat*.

* Many years ago I was accustomed to explode a mixture of chlorine and hydrogen by placing a lens in front of the electric light and a mirror behind it, causing the foci of both to coincide within the flask which contained the mixed gases. Twenty months before Dr. Akin appeared at Newcastle I used substantially the same arrangement in attempting to obtain an intense focus of invisible rays. It was my friend Dr. Debus who, in answer to my inquiry about a proper solvent for iodine, proposed bisulphide of carbon. I have since tried many other solvents, but have found none so good. Could we obtain carbon in a *state of solution*, it also might be found highly pervious to the ultra-red waves.

In the presence of these published facts, which, as far as I can see, were known to me before the name of Dr. Akin was ever associated with a physical inquiry, will it be believed that I needed this gentleman's "ideas" to inform me what I was to do with the obscure radiation from the electric light? From Dr. Akin, directly or indirectly, I never derived the fragment of an idea for the work that I have accomplished. My work would have been far more completely done, by this time, had he never existed. His value to me has been purely negative, and that to an extent little dreamt of by the readers of the Philosophical Magazine. The real fact, moreover, is, that ten months ago I performed the thankless task of communicating *my* ideas to Dr. Akin. I then told him that a solution of iodine, in a rock-salt cell, would enable him to stop the solar light with the least possible detriment to the purely thermal rays. I urged him to try the experiment; but he objected that rock-salt plates sufficiently large could not be obtained. "Bring your cell near the focus," I replied, "and you will not require large plates." He rejoined that the plates would be destroyed by the heat; I doubted this, but he finally silenced me by the remark, that he had been actually led to try the solution of iodine by what I had stated in my book regarding it, but that it would not answer. It is the use which, *to his knowledge*, I have recently made of this very substance that has roused his ire, and impelled him to the unwarrantable attack which he has made upon me in the last Number of the Philosophical Magazine.

The true motives of that attack do not at all appear upon the face of it; and this leads me to remark that it is the absence of a frank and open bearing, on the part of this gentleman, which has created difficulties between him and me. In fact, had either of us been other than he is, all difference might have been avoided. If I had been suspicious, I should have kept him at a distance; if he had been outspoken, we should have understood each other. There are words placed between inverted commas, in his "Note on Ray-transmutation," which no reader of the Philosophical Magazine can understand—the meaning of which is known to Dr. Akin and myself alone. For instance, the words "that very subject," and the word "attack" are extracted from a private letter, already referred to, as written to Dr. Akin from the Isle of Wight. He possesses that letter; *I now expressly ask him to publish it in extenso.* It is a short note, which will more clearly reveal the spirit in which I proposed to deal with this question, than anything I can now say.

At the very time when the proofs of my paper "On Luminous and Obscure Radiation," on which he has bestowed his malevolent criticism, reached my hands, Dr. Akin was con-

versing with me at the Royal Institution. I broke the cover in his presence, and, finding that it contained a duplicate proof, handed one directly to him; for I wished him to see what I had there said regarding himself. He read that proof before I did; and though this occurred ten or twelve days prior to the publication of the paper, or about the 18th of October, the first murmur of his dissatisfaction comes, at once to the public and to me, in the December Number of the Philosophical Magazine. It is not, I believe, the rule of courtesy in this country to publish private correspondence without some mutual understanding, much less to garble it. But I trust I do not offend against this rule by stating that twenty-four hours before Dr. Akin's article "On Ray-transmutation" met the public eye, I received a friendly note from that gentleman, acknowledging some trifling civilities which it had been in my power to show him, but not containing the slightest intimation of his attack. During the last days of October, and the early part of November, there had passed between Dr. Akin and myself a somewhat voluminous correspondence, which, when it ceased to be useful, I was obliged to end, with an understanding, however, that it should be renewed as soon as his feelings had calmed down. I rejoiced to think that the friendly communication above referred to was an evidence that the period of calmness had arrived, and I resolved, if such were the case, to give him an opportunity of associating his name with the experiments I had been making on the invisible radiation of the electric light. The vanity of this resolve is now demonstrated. The words "will now realize and 'publish' a discovery," used in the last page of Dr. Akin's article, are also quite characteristic. No one could infer from these words that I had actually, out of consideration for him, waived all right of making my researches known until the 3rd of November 1865, for the express purpose of giving him the chance of prior publication. I may add that when I entered into this voluntary engagement, which, by his own deliberate act, he has now dissolved, I had no notion that Dr. Akin had any doubt of his ability to give his attention to scientific researches.

The following brief summary may, perhaps, spare him the time and trouble of further criticism regarding the "inconclusiveness" of my experiments.

1. By sending the beam from the electric lamp through a sufficiently thick layer of iodine dissolved in bisulphide of carbon, the luminous portion of the radiation may be entirely intercepted, and the non-luminous almost entirely transmitted.

2. The invisible rays, suitably converged, form, at their place of convergence, a clearly defined, but perfectly invisible image of the coal-points whence the rays emanate.

3. A piece of zinc-foil placed at the focus of invisible rays, burns with its characteristic purple flame. Chemists know that there is some difficulty in causing this substance to blaze, even in a flame of high temperature.

4. Placing a thin plate of a refractory metal at this focus, a space of this metal, corresponding to the invisible image, is raised to brilliant incandescence.

5. When, instead of a metal, a sheet of carbon, placed *in vacuo*, is brought into the focus of invisible rays, the incandescent thermograph of the coal-points is also vividly formed. Cutting the sheet of carbon along the boundaries of the thermograph, we obtain a pair of incandescent coal-points, larger and less intensely illuminated than the original ones, but of the same shape. Thus, by means of the invisible rays of one pair of coal-points, we may render a second pair luminous.

6. By a suitable arrangement of the carbon terminals a metal on which their image falls may be raised to *a white heat*.

7. The light of a metal thus rendered white-hot yields, on prismatic analysis, a brilliant spectrum, which is derived wholly from the invisible rays lying beyond the extreme red of the source.

8. When the electric light is looked at, directly, through the solution employed in these experiments, nothing is seen.

9. When, in a dark room, a suitable screen is placed in the focus of invisible rays, nothing is seen.

10. When a solution of sulphate of quinine, or a piece of uranium-glass, is placed in the focus, nothing is seen.

11. When the retina of the human eye is placed at the focus, in which metal plates are raised to incandescence, nothing is seen.

The injury to my eyes, resulting from this experiment, was, I believe, less than that produced by the night-labour which the writing of this article has imposed upon me.

Royal Institution, December 1864.

IX. *Note on the History of Energy.*

By P. G. Tait, *M.A.**

IN the December Number of the Philosophical Magazine, Dr. Akin has called in question the statement that Newton, in a Scholium to his Third Law of Motion, "completely enunciated the Conservation of Energy in ordinary mechanics." He calls attention to the circumstance that the words "*in omni instrumen-*

* Communicated by the Author.

torum usu" which, for brevity, I omitted in the quotation from the *Principia*, appear to him to alter the meaning and application of the passage. Now I consider them to involve precisely that restriction ["in ordinary mechanics"] under which I made the assertion about Newton. In fact the three English words form a perfectly complete, though not literal, translation of the four Latin ones. Any rigid body, subject to such forces as pressures, gravitation, &c., is really a machine—whether it be employed for mechanical purposes or not. I took care to indicate the omission of this qualifying clause, though it had, in fact, been supplied in my general statement.

I regret that the Treatise on Natural Philosophy, on which Prof. W. Thomson and I have been for a long time engaged, is not yet published. The portion bearing on my present subject was printed off considerably more than a year ago. I shall not, however, quote from it, but from a 'Sketch of Elementary Dynamics'* published in October 1863 for the use of students in Glasgow and Edinburgh. In that pamphlet—after quoting Newton's memorable words—we proceed (p. 30),

"In a previous discussion Newton has shown what is to be understood by the velocity of a force or resistance; *i. e.*, that it is the velocity of the point of application of the force *resolved in the direction of the force*, in fact proportional to the virtual velocity. Bearing this in mind, we may read the above statement as follows:—

"*If the action of an agent be measured by its amount and its velocity conjointly; and if, similarly, the Reaction of the resistance be measured by the velocities of its several parts and their several amounts conjointly, whether these arise from friction, cohesion, weight, or acceleration;—Action and Reaction, in all combinations of machines, will be equal and opposite.*"

We then show, in passing, that D'Alembert's principle is distinctly pointed out, and proceed thus (p. 31):

"The foundation of the abstract theory of energy is laid by Newton in an admirably distinct and compact manner in the sentence of his scholium already quoted, in which he points out its application to mechanics. The *actio agentis*, as he defines it, which is evidently equivalent to the product of the effective component of the force, into the velocity of the point on which it acts, is simply, in modern English phraseology, the rate at which the agent works. The subject for measurement here is precisely the same as that for which Watt, a hundred years later, introduced the practical unit of a '*Horse-power*,' or the rate at which an agent works when overcoming 33,000 times the

* Edinburgh: Maclachlan and Stewart. Pp. 44.

weight of a pound through the space of a foot in a minute; that is, producing 550 foot-pounds of work per second. The unit, however, which is most generally convenient is that which Newton's definition implies, namely, the rate of doing work in which the unit of energy is produced in the unit of time.

"Looking at Newton's words in this light, we see that they may be logically converted into the following form:—

" 'Work done on any system of bodies (in Newton's statement, the parts of any machine) has its equivalent in work done against friction, molecular forces, or gravity, if there be no acceleration; but if there be acceleration, part of the work is expended in overcoming the resistance to acceleration, and the additional kinetic energy developed is equivalent to the work so spent.'

" When part of the work is done against molecular forces, as in bending a spring; or against gravity, as in raising a weight; the recoil of the spring, and the fall of the weight, are capable, at any future time, of reproducing the work originally expended. But in Newton's day, and long afterwards, it was supposed that work was *absolutely lost* by friction."

This shows that, so far as experimental facts were known in Newton's time, he had the Conservation of Energy complete; the cases of apparent loss by impact, friction, &c. were not then understood.

The opinion of James Bernoulli on a question of this nature would undoubtedly be valuable, but he seems not to have noticed Newton's remark. But I must protest against the allowing any weight to that of John Bernoulli, who, while inferior to his brother as a mathematician, was so utterly ignorant of the principle in question as seriously to demonstrate the possibility of a perpetual motion, founded on the alternate mixing of two liquids and their separation by means of a filter.

I take this opportunity of mentioning, with reference to Mr. Monro's paper in the December Number of the Philosophical Magazine, that in the very paper by Professor Thomson in which the word "naturalist" is used (after Johnson) for Natural Philosopher; Dynamics is divided, as Mr. Monro suggests it should be, into Statics and Kinetics. The same division is employed in the pamphlet above quoted from.

6 Greenhill Gardens, Edinburgh,
December 13, 1864.

X. *On Thermal Radiation.* *By* Prof. Magnus*.

In a previous "Note on the Constitution of the Sun"† I communicated the results of some experiments on the thermal radiating powers of sodium, lithium, potassium, &c. These experiments were made so as to compare the radiation of a platinum plate, heated in a Bunsen's burner, with that of an exactly equal and similar plate covered with fused carbonate of soda, of lithia, &c. The great radiating power of these substances appeared to be due not improbably to the circumstance that, at the high temperature to which they are exposed, small particles continually detach themselves, which particles produce the intense and peculiarly coloured light of the flame. At the moment of detachment these particles may be regarded as so many points; so that it appeared possible that the radiation might be determined by them or by the roughness of the surfaces of the glowing substances. It is well known, in fact, that rough metallic surfaces do radiate more heat than smooth ones—either in consequence of the points which they present, or, as Melloni‡ and Knoblauch§ assert, because their density when rough is smaller than when they are smooth.

In order to determine whether the greater radiation of sodium and of other similar substances does in reality depend upon the detachment of such small particles, their radiating powers at 100° C. were compared with that of platinum. For this purpose a small apparatus was expressly constructed; it was heated by steam, and to its radiating surface, which had a diameter of 22 millimetres, several plates could successively be attached. The result was, that even at 100° C. a platinum plate covered with fused sodium was found to radiate much more heat than a platinum plate not so covered. It is difficult to obtain exact measurements of the relative magnitudes of these radiations, since the cohesion or tendency to the formation of drops is so strong in carbonate of soda that the substance readily flows to one place on the plate, and not only refuses to distribute itself uniformly over the latter, but frequently recedes altogether from certain patches. Nevertheless the experiments were sufficiently accurate to prove that the radiating powers of platinum and sodium have a similar relation to each other at 100°, to that which they have at the temperature of a Bunsen's flame.

It follows from this that the great radiating power of sodium,

* From the *Monatsberichte* for August 11, 1864.

† *Monatsberichte* for 1864, p. 166. Poggendorff's *Annalen*, vol. cxxi. p. 510. Phil. Mag. S. 4. vol. xxvii. p. 376.

‡ *Thermochrose*, p. 90, Remark.

§ Poggendorff's *Annalen*, vol. lxx. p. 340.

and of other like substances, does not arise from the particles detached at a glowing heat.

The radiating power of these particles is at most very small, in fact much less important than is stated in the "Note on the Constitution of the Sun" above referred to. New experiments have shown that when all precautions are taken, so that none of the rays proceeding from solid bodies which do not belong to the flame can reach the pile, the luminous soda-flame scarcely radiates more than the non-luminous flame. The particles of soda in the flame suffice, it is true, to increase its luminosity; but their mass is too small to augment to any great extent the radiation of heat.

Although these particles are entirely absent at a temperature of 100°, the radiation of sodium relative to that of platinum is just as great when both bodies have this temperature as it is when they are both raised to a glowing heat.

Moreover the radiation of platinum itself varies greatly. When a smooth platinum plate is covered with spongy platinum, either by applying strong heat after pouring a solution of ammonio-platinum on the plate, or by precipitating platinum thereon by a galvanic current, its radiating power becomes so great as to equal, and sometimes even to exceed that of sodium.

In the experiments with a Bunsen's flame we might attribute this augmented radiation to the fact that the spongy platinum, owing to its porosity, assumes a higher temperature than does the smooth plate with which the former is compared; the fact, however, is, that even at a temperature of 100° a plate covered with spongy platinum radiates more heat than a smooth one, and that in the same ratio as when the two are heated in a flame. At 100°, however, when the posterior surface of the plate is warmed, the spongy platinum can in no case be warmer than the plate from which it receives its heat.

When the spongy platinum is submitted to the pressure of a polishing-iron its radiating power diminishes, and this radiation continues to diminish according as the sponginess is caused more and more to disappear by pressing, hammering, or in any other manner increasing the density. This circumstance, however, cannot be regarded as a proof that the radiation depends upon the density rather than upon the roughness; for as the density of the spongy platinum increases, the roughness of its surface diminishes.

The radiation of so-called platinum-black appeared on this account worthy of examination. This substance, as is well known, consists of platinum solely; its molecular constitution, however, differs from that of spongy platinum, its particles being far more finely distributed. This substance, which can

only be employed at low temperatures, was difficult to fix, in consequence of its being heated. I could find no better method of securing this fixity than to press platinum-black lightly on a platinum plate which had previously been smeared uniformly with a very thin layer of fat. On afterwards shaking the plate, the black adheres to it uniformly. Plates so prepared radiate about 25 per cent. more heat than when they are covered with spongy platinum.

Lampblack, when distributed in a similar manner over a platinum plate, has a radiating power quite similar to that of platinum-black. Whether the thermal colours of the two are the same or different was not examined.

The question whether the magnitude of the radiation is or is not determined by the roughness of the surface is an exceedingly important one in every theory which refers the phenomena of heat to motion. Melloni and Knoblauch, in the memoirs already cited, assert that the radiation depends, not on the form (*Gestalt*) of the surface, but upon its density solely. None of the ingenious experiments upon which Melloni bases his assertion, is more conclusive than his observation that rough surfaces do not *invariably* radiate more than smooth ones; for example, marble, jet, ivory, quartz, gypsum, and some other substances do not, according to this philosopher, radiate more heat when in a rough than when in a smooth condition. I have found, too, that both the white and black varieties of mica (*Glimmer von Miask*) as well as several non-metallic substances, deport themselves in a similar manner. Alum in a finely powdered condition fuses at 100°, and its fused surface radiates almost the same as does the rough powder. Powdered sugar scarcely radiates more than fused sugar.

On the other hand, Melloni admits that metals in the state of chemical precipitates (*e. g.* when distributed in a finely divided state over the surface of a Leslie's cube) possess very great radiating powers.

Filings (*Feilspähne*) deport themselves in the same manner. They, too, increase the radiation very considerably when distributed over the rough surface of the same metal. In order to meet every objection against this experiment, I spread the filings upon so thin a platinum plate that their most prominent points were not nearer to the thermo-electric pile than was the anterior surface of the thicker rough platinum plate with which the filings were compared. This plate, too, was cut from the same piece of metal which furnished the filings.

An experiment with aluminium gave like results. This metal in the rough, as well as in the smooth condition, is a better radiator of heat than either platinum or silver.

Melloni strives also to refer the greater radiation of powdered bodies, wherever it occurs, to a difference in density. He holds that the separated surfaces of the small particles are less dense than is the smooth continuous surface of the same metal. This view is scarcely tenable. It will scarcely be asserted that filings are less dense than the rough surface of the metal from which they are obtained; nor will it be conceded that platinum-black is less dense than spongy platinum, but merely that it is in a state of greater distribution. We shall consequently be compelled to assume that in metals the degree of distribution as well as the density exerts an influence on the radiating power. Whatever may be the condition of the metal, however, which gives the greatest radiation, whether it be one of less density or greater distribution of the substance, we are always compelled to admit that the vibrating particles of the body, and the æther which immediately surrounds them, are less able to communicate their motion to the external æther when the surface of the metal is smooth than when this surface is rough, or less dense, or when the substance on the surface is in a condition of greater distribution.

An hypothesis might certainly be found to account for the fact that change of density and distribution alter so greatly the communication of the above motion; and from such an hypothesis a simple connexion could be easily deduced between the radiation, absorption, diathermancy, and thermal conductibility of bodies.

XI. *On the Work or Potential of Torsion. New Method of establishing the Equations which regulate the Torsion of Elastic Prisms.* By M. de Saint-Venant*.

THE general expression for the potential of elasticity per unit of volume-element, that is to say, for the molecular work Φ which a deformed element of an elastic body is capable of yielding during its detorsion or return to its natural and primitive state —in other words, the formula for the external work performed in forcing this element from its natural state to the actually supposed state of deformation or tension—is

$$\Phi=\tfrac{1}{2}(p_{xx}\,\delta_x+p_{yy}\,\delta_y+p_{zz}\,\delta_z+p_{yz}\,g_{yz}+p_{zx}\,g_{zx}+p_{xy}\,g_{xy}), \quad . \quad (1)$$

where $p_{xx}\ldots p_{xy}$ denote the six components, parallel to the rectangular coordinate axes, of the pressures per unit of surface, on three small faces drawn through the centre of the element per-

* From the *Comptes Rendus*, November 14, 1864.

pendicular, respectively, to the coordinates x, y, z; $\vartheta_x, \vartheta_y, \vartheta_z$ the *dilatations*, that is to say, the relative elongations of the sides of the elementary parallelopiped parallel to x, y, z; and g_{yz}, g_{zx}, g_{xy} the relative *slidings*, one over the other and per unit of their distance, of the opposite sides; in other words, the cosines of the three slightly acute angles formed, after the deformation, by the three pairs of adjacent sides of the parallelopiped*.

On substituting the values of the six components p, each of which, as is known, is expressible as a linear function of the six elementary and very small deformations ϑ, g, an expression for Φ, of the second degree, is obtained which contains twenty-one terms, involving the squares and products of these quantities. When, as in the case of the simple torsion of a prism around a longitudinal axis parallel to that of x, the slidings g_{xy}, g_{xz} alone enter into consideration, this expression reduces itself to

$$\Phi = \tfrac{1}{2}(\mathrm{G} g_{xy}^2 + \tfrac{1}{2}\mathrm{G}' g_{xz}^2), \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where G and G′ are the coefficients of elasticity of sliding in the transverse directions of y and z. It may moreover be demonstrated that each of the terms of the last expression is equal to the sum of the quantities of work corresponding to a dilatation $\frac{1}{2}g_{xy}$ or $\frac{1}{2}g_{xz}$, and to an equal contraction in the directions, inclined to each other at an angle of 45°, of the bisectors of the right angles enclosed by the axes of x, y and those of x, z†.

The potential of torsion for the volume corresponding to the unit of length of the prism is equal to the integral of the above quantity, previously multiplied by $dy\,dz$, extended over the surface of a transverse section.

The *moment of torsion* M, however—that is to say, the external transverse force capable, when acting at the extremity of a lever whose length is unity, of maintaining around the longitudinal axis a certain acquired torsion θ which shall develope on the section the tangential components of pressure p_{xy}, p_{xz}—has manifestly the value

$$\mathrm{M} = \int\int dy\,dz(p_{xz}\,y - p_{xy}\,z); \quad . \quad . \quad . \quad . \quad (3)$$

and if the force thus applied increases from 0 to the value M, or until the torsion, per unit of length of the prism, has attained the magnitude θ, which latter is that of the angle described by

* See *Comptes Rendus*, 1861, vol. liii. p. 1108, or the formula 200 in the complementary appendix to the new (1864) annotated edition of the *Leçons* of Navier.

† See the annotated *Leçons* of Navier, second note of No. 42 of the historical part.

one edge of the base of the prism, the work performed will be

$$M\frac{\theta}{2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

In order that this expression may be equal to the one obtained by substituting the value of Φ, given in (2), in the integral

$$\iint \Phi dy\, dz,$$

the following condition must be satisfied identically:

$$\tfrac{1}{2}\iint dy\, dz(G g_{xy}^2 + G' g_{xz}^2) = \frac{\theta}{2}\iint dy\, dz(G' g_{xz}\, y - G g_{xy}\, z); \quad (5)$$

provided we limit ourselves to the sufficiently general hypothesis of three planes of symmetry of contexture perpendicular to the axes of x, y, z, in which case

$$p_{xy} = G g_{xy}, \quad p_{xz} = G' g_{xz}. \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Now, since for the torsion we have

$$g_{xy} = \frac{du}{dy} - \theta z, \quad g_{xz} = \frac{du}{dz} + \theta y, \quad . \quad . \quad . \quad (7)$$

on denoting by u the *longitudinal* displacement parallel to x of the point (y, z) of the section, the equation (5) becomes, by substitution and reduction,

$$\iint dy\, dz\left[G\frac{du}{dy}\left(\frac{du}{dy} - \theta z\right) + G'\frac{du}{dz}\left(\frac{du}{dz} + \theta y\right)\right] = 0. \quad . \quad (8)$$

If we integrate, partially, the first and second terms according to y and z respectively, we shall detach the simple integrals

$$\left[\int dz\, G u\left(\frac{du}{dy} - \theta z\right)\right]_0^1, \quad \left[\int dy G' u\left(\frac{du}{dz} + \theta y\right)\right]_0^1, \quad . \quad (8a)$$

where the indices 0, 1 indicate that the difference between the two values of the $\int$ must be taken relative to the two intersections of the contour of the section by a parallel to y, and by a parallel to z. At these points we have

$$dz = \pm ds \cos(n, y), \quad dy = \pm ds \cos(n, z),$$

where ds denotes the arc-element of the contour and n the direction of its externally drawn normal; so that the two differences (8*a*) become sums of the form $\int ds \ldots$, extended over the *whole con-*

tour. By this well-known mode of transformation, first employed by Lagrange, the equation (8) to be verified becomes

$$\left.\begin{aligned}\int u\,ds\left[\mathrm{G}\left(\frac{du}{dy}-\theta z\right)\cos(n,y)+\mathrm{G}'\left(\frac{du}{dz}+\theta y\right)\cos(n,z)\right]\\ -\theta\iint u\,dy\,dz\left[\mathrm{G}\,\frac{d^2u}{dy^2}+\mathrm{G}'\,\frac{d^2u}{dz^2}\right]=0.\end{aligned}\right\}. \quad (9)$$

Now the squares of the second, and of the first parenthesis, each equated to zero, give precisely and respectively the indefinite differential equation applicable to all points of a section, and the definite differential equation having reference to points on the contour; which equations I established in 1847 and in 1853, and presented* as containing, implicitly, the whole theory of the torsion of prisms having any base whatever and composed of matter whose contexture was doubly symmetrical, as in the case to which, for simplicity, we have limited ourselves in the above considerations. Moreover, in the several cases of circular, elliptic, triangular, equilateral, &c. sections, I obtained by calculation the same value for the potential of torsion from both its expressions, $\iint\Phi\,dy\,dz$ and $\mathrm{M}\frac{\theta}{2}$.

It will be seen that the consideration of the potential or work of elasticity completely verifies the general and special results to which I was led, in a different manner, on establishing the theory of torsion. This consideration, moreover, is evidently connected with the methods of the *Mécanique Analytique* employed by Navier in 1821, and recently verified again by several mathematical physicists, amongst whom may be mentioned the regretted Clapeyron; for his theorem, after supplying the forgotten factor $\frac{1}{2}$, resolves itself to a particular case of the equation (1).

The calculation of the potential of torsion has also, in itself, a practical value; for the helical springs frequently opposed to shocks of different kinds, work almost wholly by the torsion of their threads, as I showed in 1843, and as was also remarked by Binet in 1814, and M. Giulio in 1840, and recently by railway engineers.

* *Mémoires des Savants Etrangers*, vol. xiv.; or note to the No. 156 of the *Leçons* of Navier.

XII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from vol. xxviii. p. 479.]

June 16, 1864.—Major-General Sabine, President, in the Chair.

THE following communication was read :—

"Further Inquiries concerning the Laws and Operation of Electrical Force." By Sir W. Snow Harris, F.R.S., &c.

1. The author first endeavours to definitely express what is meant by *quantity of electricity*, *electrical charge*, and *intensity*.

By *quantity of electricity* he understands the actual amount of the unknown agency constituting electrical force, as represented by some arbitrary quantitative 'electrical' measure. By *electrical charge* he understands the quantity which can be sustained upon a given surface under a given electrometer indication. *Electrical intensity*, on the contrary, is 'the electrometer indication' answering to a given quantity upon a given surface.

2. The experiments of Le Monnier in 1746, of Cavendish in 1770, and the papers of Volta in 1779, are quoted as showing that bodies do not take up electricity in proportion to their surfaces. According to Volta, any plane surface extended in length sustains a greater charge—a result which this distinguished philosopher attributes to the circumstance that the electrical particles are further apart upon the elongated surface, and consequently further without each other's influence.[1]

3. The author here endeavours to show that, in extending a surface in length, we expose it to a larger amount of inductive action from surrounding matter, by which, on the principles of the condenser, the intensity of the accumulation is diminished, and the charge consequently increased; so that not only are we to take into account the influence of the particles on each other, but likewise their operation upon surrounding matter.

4. No very satisfactory experiments seem to have been instituted showing the relation of quantity to surface. The quantity upon a given surface has been often vaguely estimated without any regard to a constant electrometer indication or intensity. The author thinks we can scarcely infer from the beautiful experiment of Coulomb, in consequence of this omission, that the capacity of a circular plate of twice the diameter of a given sphere is twice the capacity of the sphere, and endeavours to show, in a future part of the paper (Experiment 16), that the charge of the sphere and plate are to each other not really as 1·2, but as $1:\sqrt{2}$, that is, as the square roots of the exposed surfaces; so that we cannot accumulate twice the quantity of electricity upon the plate under the same electrometer indication.

5. On a further investigation of the laws of electrical charge, the quantity which any plane rectangular surface can receive under a given intensity is found to depend not only on the surface, but also on its linear boundary extension. Thus the linear boundary of 100 square inches of surface under a rectangle 37·5 inches long by 2·66 inches

wide, is about 80 inches ; whilst the linear boundary of the same 100 square inches of surface under a plate 10 inches square is only 40 inches. Hence the charge of the rectangle is much greater than that of the square, although the surfaces are equal, or nearly so.

6. The author finds, by a rigid experimental examination of this question, that electrical charge depends upon surface and linear extension conjointly. He endeavours to show that there exists in every plane surface what may be termed an electrical boundary, having an important relation to the grouping or disposition of the electrical particles in regard to each other and to surrounding matter. This boundary, in circles or globes, is represented by their circumferences. In plane rectangular surfaces, it is their linear extension or perimeter.

If this *boundary* be constant, their electrical charge (1) varies with the square root of the surface. If the *surface* be constant, the charge varies with the square root of the boundary. If the surface and boundary both vary, the charge varies with the square root of the surface multiplied into the square root of the boundary. Thus, calling C the charge, S the surface, B the boundary, and μ some arbitrary constant depending on the electrical unit of charge, we have $C=\mu\sqrt{S.B}$, which will be found, with some exceptions, a general law of electrical charge. It follows from this formula, that if when we double the surface we also double the boundary, the charge will be also double. In this case the charge may be said to vary with the surface, since it varies with the square root of the surface, multiplied into the square root of the boundary. If therefore the surface and boundary both increase together, the charge will vary with the square of either quantity. The quantity of electricity therefore which surfaces can sustain under these conditions will be as the surface. If l and b represent respectively the length and breadth of a plane rectangular surface, then the charge of such a surface is expressed by $\mu\sqrt{2lb\ (l+b)}$, which is found to agree perfectly with experiment. We have, however, in all these cases to bear in mind the difference between *electrical charge* and *electrical intensity* (1).

7. The electrical intensity of plane rectangular surfaces is found to vary in an inverse ratio of the boundary multiplied into the surface. If the surface be constant, the intensity is inversely as the boundary. If the boundary be constant, the intensity is inversely as the surface. If both vary alike and together, the intensity is as the square of either quantity ; so that if when the surface be doubled the boundary be also doubled, the intensity will be inversely as the square of the surface. The intensity of a plane rectangular surface being given, we may always deduce therefrom its electrical charge under a given greater intensity, since we only require to determine the increased quantity requisite to bring the electrometer indication up to the given required intensity. This is readily deduced, the intensity being, by a well-established law of electrical force, as the square of the quantity.

8. These laws relating to charge, surface, intensity, &c., apply more especially to continuous surfaces taken as a whole, and not to surfaces divided into separated parts. The author illustrates this by examining the result of an electrical accumulation upon a plane

rectangular surface taken as a whole, and the results of the same accumulation upon the same surface divided into two equal and similar portions distant from each other, and endeavours to show, that if as we increase the quantity we also increase the surface and boundary, the intensity does not change. If three or more separated equal spheres, for example, be charged with three or more equal quantities, and be each placed in separate connexion with the electrometer, the intensity of the whole is not greater than the intensity of one of the parts. A similar result ensues in charging any united number of equal and similar electrical jars. A battery of five equal and similar jars, for example, charged with a given quantity $=1$, has the same intensity as a battery of ten equal and similar jars charged with quantity $=2$; so that the intensity of the ten jars taken together is no greater than the intensity of one of the jars taken singly. In accumulating a double quantity upon a given surface divided into two equal and separate parts, the boundaries of each being the same, the intensity varies inversely as the square of the surface. Hence two separate equal parts can receive, taken together under the same electrometer indication, twice the quantity which either can receive alone, in which case the charge varies with the surface. Thus if a given quantity be disposed upon two equal and similar jars instead of upon one of the jars only, the intensity upon the two jars will be only one-fourth the intensity of one of them, since the intensity in this case varies with the square of the surface inversely, whilst the quantity upon the two jars under the same electrometer indication will be double the quantity upon one of them only; in which case the charge varies with the surface, the intensity being constant. If therefore as we increase the number of equal and similar jars we also increase the quantity, the intensity remains the same, and the charge will increase with the number of jars. Taking a given surface therefore in equal and divided parts, as for example four equal and similar electrical jars, the intensity is found to vary with the square of the quantity directly (the number of jars remaining the same), and with the square of the surface inversely (the number of jars being increased or diminished); hence the charge will vary as the square of the quantity divided by the square of the surface; and we have, calling C the charge, Q the quantity, and S the surface, $C=\frac{Q^2}{S^2}$; which formula fully represents the phenomenon of a constant intensity, attendant upon the charging of equal separated surfaces with quantities increasing as the surfaces; as in the case of charging an increasing number of equal electrical jars. Cases, however, may possibly arise in which the intensity varies inversely with the surface, and not inversely with the square of the surface. In such cases, of which the author gives some examples, the above formula does not apply.

9. From these inquiries it is evident, as observed by the early electricians, that conducting bodies do not take up electricity in proportion to their surfaces, except under certain relations of surface and boundary. If the breadth of a given surface be indefinitely diminished,

and the length indefinitely increased, the surface remaining constant, then, as observed by Volta, the least quantity which can be accumulated under a given electrometer indication is when the given surface is a circular plate, that is to say, when the boundary is a minimum, and the greatest when extended into a right line of small width, that is, when the boundary is a maximum. In the union of two similar surfaces by a boundary contact, as for example two circular plates, two spheres, two rectangular plates, &c., we fail to obtain twice the charge of one of them taken separately. In either case we fail to decrease the intensity (the quantity being constant) or to increase the charge (the intensity being constant), it being evident that whatever decreases the electrometer indication or intensity must increase the charge, that is to say, the quantity which can be accumulated under the given intensity. Conversely, whatever increases the electrometer indication decreases the charge, that is to say, the quantity which can be accumulated under the given intensity.

10. If the grouping or disposition of the electrical particles, in regard to surrounding matter, be such as not to materially influence external induction, then the boundary extension of the surface may be neglected. In all similar figures, for example, such as squares, circles, spheres, &c., the electrical boundary is, in relation to surrounding matter, pretty much the same in each, whatever be the extent of their respective surfaces. In calculating the charge, therefore, of such surfaces, the boundary extensions may be neglected, in which case their relative charges are found to be as the square roots of the surfaces only ; thus the charges of circular plates and globes are as their diameters, the charges of square plates are as their sides. In rectangular surfaces also, having the same boundary extensions, the same result ensues, the charges are as the square roots of the surfaces. In cases of hollow cylinders and globes, in which one of the surfaces is shut out from external influences, only one-half the surface may be considered as exposed to external inductive action, and the charge will be as the square root of half the surface, that is to say, as the square root of the exposed surface. If, for example, we suppose a square plate of any given dimensions to be rolled up into an open hollow cylinder, the charge of the cylinder will be to the charge of the plate into which we may suppose it to be expanded as $1 : \sqrt{2}$. In like manner, if we take a hollow globe and a circular plate of twice its diameter, the charge of the globe will be to the charge of the plate also as $1 : \sqrt{2}$, which is the general relation of the charge of closed to open surfaces of the same extension. The charge of a square plate to the charge of a circular plate of the same diameter was found to be $1 : 1{\cdot}13$; according to Cavendish it is as $1 : 1{\cdot}15$, which is not far different. It is not unworthy of remark that the electrical relation of a square to a circular plate of the same diameter, as determined by Cavendish nearly a century since, is in near accordance with the formulæ $C = \sqrt{S}$ above deduced.

11. The author enumerates the following formulæ as embracing the general laws of quantity, surface, boundary extension, and intensity, practically useful in deducing the laws of statical electrical force.

Symbols.

Let C = electrical charge; Q = quantity; E = intensity, or electrometer indication; S = surface, B = boundary extension, or perimeter; Δ = direct induction; δ = reflected induction; F = force; D = distance.

Formulæ.

$C \propto S$, when S and B vary together.
$C \propto Q$, E being constant, or equal 1.
$C \propto \sqrt{S}$, B being constant, or equal 1.
$C \propto \sqrt{B}$, S being constant, or equal 1.
$C \propto \sqrt{S}.B$, when S and B vary together.
$E \propto \frac{1}{S.B}$ (Q being constant), for all plane rectangular surfaces.
$E \propto \frac{1}{B}$, S being constant, or equal 1.
$E \propto \frac{1}{S}$, B being constant, or equal 1.
$E \propto \frac{1}{S^2}$, when S and B vary together.
$C \propto \sqrt{\frac{1}{E}}$. . . . . .
$E \propto Q^2$, S being constant, or equal 1.
$C \propto \frac{Q^2}{S^2}$. . . . . .

In square plates, $C \propto$ with side of square.
In circular plates, $C \propto$ with diameter.
In globes, $C \propto$ with diameter.
Δ, or induction $\propto S$, all other things remaining the same.
The same for δ, or reflected induction.
In circular plates, globes, and closed and open surfaces,

$E \propto \frac{1}{S}$; or as $\frac{1}{\Delta}$.
$F\ (=E) \propto Q^2$.
F or $E \propto \frac{1}{D^2}$, S being constant.

Generally we have $F \propto \frac{Q^2}{D^2}$.

12. The author calculates from these laws of charge for circles and globes a series of circular and globular measures of definite values, taking the circular inch or globular inch as unity, and calling, after Cavendish, a circular plate of an inch in diameter, charged to saturation, a circular inch of electricity; or otherwise charged to any degree short of saturation, a circular inch of electricity under a given intensity. In like manner he designates a globe of an inch in diameter a globular inch of electricity.

In the following Table are given the quantities of electricity con-

tained in circular plates and globes, together with their respective intensities for diameters varying from ·25 to 2 inches; a circular plate of an inch diameter and $\frac{1}{5}$th of an inch thick being taken as unity, and supposed to contain 100 particles or units of charge.

Diameters, or units of charge.	Circle.		Globe.	
	Particles.	Intensity.	Particles.	Intensity.
0·25	25	0·062	35	0·124
0·50	50	0·250	70	0·500
0·75	75	0·560	105	1·120
1·00	100	1·000	140	2·000
1·25	125	1·560	175	3·120
1·40	140	1·960	196	3·920
1·50	150	2·250	210	4·500
1·60	160	2·560	224	5·120
1·75	175	3·060	245	6·120
2·00	200	4·000	280	8·000

13. The experimental investigations upon which these elementary data depend, constitute a second part of this paper. The author here enters upon a brief review of his hydrostatic electrometer, as recently perfected and improved, it being essential to a clear comprehension of the laws and other physical results arrived at.

In this instrument the attractive force between a charged and neutral disk, in connexion with the earth, is hydrostatically counterpoised by a small cylinder of wood accurately weighted, and partially immersed in a vessel of water. The neutral disk and its hydrostatic counterpoise are freely suspended over the circumference of a light wheel of 2·4 inches in diameter, delicately mounted on friction-wheels, so as to have perfectly free motion, and be susceptible of the slightest force added to either side of the balance. Due contrivances are provided for measuring the distance between the attracting disks. The balance-wheel carries a light index of straw reed, moveable over a graduated quadrantal arc, divided into 90° on each side of its centre. The neutral attracting plate of the electrometer is about $1\frac{1}{2}$ inch in diameter, and is suspended from the balance-wheel by a gold thread, over a similar disk, fixed on an insulating rod of glass, placed in connexion with any charged surface the subject of experiment. The least force between the two disks is immediately shown by the movement of the index over the graduated arc in either direction, and is eventually counterpoised by the elevation or depression in the water of the hydrostatic cylinder suspended from the opposite side of the wheel. The divisions on the graduated quadrant correspond to the addition of small weights to either side of the balance, which stand for or represent the amount of force between the attracting plates at given measured distances, with given measured quantities of electricity. This arrangement is susceptible of very great accuracy of measurement.

The experiment requires an extremely short time for its development, and no calculation is necessary for dissipation. The author

carefully describes the manipulation requisite in the use of this instrument, together with its auxiliary appendages. He considers this electrometer, as an instrument of electrical research, quite invaluable, and peculiarly adapted to the measurement of electrical force.

14. Having fully described this electrometer, and the nature of its indications, certain auxiliary instruments of quantitative measure, to be employed in connexion with it, are next adverted to.

First, the construction and use of circular and globular transfer measures given in the preceding Table, by which given measured quantities of electricity may be transferred from an electrical jar (charged through a unit-jar from the conductor of an electrical machine) to any given surface in connexion with the electrometer. The electrical jar he terms a *quantity-jar*, the construction and employment of which is minutely explained, as also the construction and employment of the particular kind of unit-jar he employs.

15. Two experiments (1 and 2) are now given in illustration of this method of investigation.

Experiment 1 developes the law of attractive force as regards quantity; which is found to vary with the square of the number of circular or globular inches of electricity, transferred to a given surface in connexion with the fixed plate of the electrometer, the distance between the attracting surfaces being constant.

Experiment 2 demonstrates the law of force as regards distance between the attracting surfaces, the quantity of electricity being constant; and by which it is seen that the force is in an inverse ratio of the square of the distance between the attracting plates, the plates being susceptible of perfect inductive action. From these two experiments, taken in connexion with each other, we derive the following formula, $F \propto \frac{Q^2}{D^2}$; calling F the force, Q the quantity, and D the distance. It is necessary, however, to observe that this formula only applies to electrical attractive force between a charged and neutral body in connexion with the earth, the two surfaces being susceptible of free electrical induction, both direct and reflected.

16. The author now refers to several experiments (3, 4, 5, and 6), showing that no sensible error arises from the reflected inductive action of the suspended neutral disk of the electrometer, or from the increased surface attendant on the connexion of the surface under experiment with the fixed plate of the electrometer; as also that it is of no consequence whether the suspended disk be placed immediately over the fixed attracting plate of the electrometer, or over any point of the attracting surface in connexion with it.

17. Having duly considered these preliminary investigations, the author now proceeds to examine experimentally the laws of surface and boundary as regards plane rectangular surfaces, and to verify the formulæ $C=\sqrt{S.B}$, and $E=\frac{1}{S.B}$; in which C=charge, E=intensity, S=surface, and B=boundary.

For this purpose a series of smoothly-polished plates of copper were employed, varying from 10 inches square to 10 inches long by

2·5 to 6 inches wide, and about $\frac{1}{6}$th of an inch thick, exposing from 100 to 200 square inches of surface.

The charges (1) of these plates were carefully determined under a given electrometer indication, the attracting plates being at a constant distance.

Experiment 7. In this experiment, a copper plate 10 inches square is compared with a rectangular plate 40 inches long by 2·5 inches wide.

In these plates the surfaces are each 100 square inches, whilst the boundaries are 40 and 85 inches The boundaries may be taken, without sensible error, as 1 : 2, whilst the surfaces are the same.

On examining the charges of these plates, charge of the square plate was found to be 7 circular inches, under an intensity of 10°. Charge of the rectangular plate 10 circular inches nearly, under the same intensity of 10°. The charges therefore were as 7 : 10 nearly, that is, as 1 : 1·4 nearly, being the square roots of the boundaries, that is, as 1 : $\sqrt{2}$.

Experiment 8. A rectangular plate 37·5 inches long by 2·7 inches wide, surface 101 square inches, boundary 80·5 inches, compared with a rectangular plate 34·25 inches long by 6 inches wide, surface 205 square inches, boundary 80·5 inches.

Here the boundaries are the same, whilst the surfaces may be taken as 1 : 2.

On determining the charges of these plates, charge of the rectangular plate, surface 101 square inches was found to be 8·5 circular inches under an intensity of 8°. Charge of the plate with double surface =205 square inches, was found to be 12 circular inches under the same intensity of 8°; that is to say, whilst the surfaces are as 1 : 2, the charges are as 8·5 : 12 nearly, or as the square roots of the surfaces, that is, as 1 : $\sqrt{2}$.

Experiment 9. A rectangular plate 26·25 inches long by 4 inches wide, surface 105 square inches, boundary 60·5, compared with a rectangular plate 40 inches long by 5 inches wide, surface 200 square inches, boundary 90 inches.

Here the surfaces are as 1 : 2 nearly, whilst their boundaries are as 2 : 3.

Charge of the rectangular plate surface =105 square inches, 7 circular inches under an intensity of 10°. Charge of rectangular plate surface 200 square inches, 12 circular inches, under the same intensity of 10°. The charges therefore are as 7 : 12 nearly, or as 1 : 1·7, being as the square roots of the surfaces multiplied into the square roots of the boundaries very nearly.

Experiment 10. A square plate 10 inches square, surface 100 square inches, boundary 40 inches, compared with a rectangular plate 40 inches long by 5 inches wide, surface 200 square inches, boundary 90 inches.

Here the surfaces are double of each other, and the boundaries also double each other, or so nearly as to admit of their being considered double of each other. Charge of square plate 6 circular inches, under an intensity of 10°. Charge of rectangular plate 12 circular inches, under the same intensity of 10°. The charges,

therefore, are as the square roots of the surfaces and boundaries conjointly, according to the formula $C = \sqrt{S.B}$, as also verified in the preceding experiment 9.

A double surface, therefore, having a double boundary, takes a double charge, but not otherwise. Neglecting all considerations of the boundary, therefore, the surface and boundary varying together, the charge in this case will be as the surface directly.

18. The author having verified experimentally the laws of surface and boundary, as regards plane rectangular surfaces, proceeds to consider the charges of square plates, circular plates, spheres, and closed and open surfaces generally.

Experiment 11. Plate 10 inches square, surface 100 square inches, boundary 40 inches, compared with a similar plate 14 inches square, surface 196 square inches, boundary 56 inches. Here the surfaces are as 1 : 2 nearly, whilst the boundaries are as $1 : \sqrt{2}$ nearly.

In this case charge of square plate, surface 100 square inches, was found to be 8 circular inches under an intensity of 10°. Charge of the plate, surface 196 square inches, 11 circular inches, under the same intensity of 10°. Here the charges are as 8 : 11, whilst the surfaces may be taken as 1 : 2, that is to say (neglecting the boundary), the charges are as the square roots of the surfaces, according to the formula $C = \sqrt{S}$.

On examining the intensities of these plates, they were found to be inversely as the surfaces; thus 8 circular inches upon the plate surface 100, evinced an intensity of 10°; 8 circular inches upon the plate, surface 196, evinced an intensity of 5° only, or $\frac{1}{2}$ the former, according to the formula $E = \frac{1}{S}$.

Experiment 12. A circular plate of 9 inches diameter, surface 63·6 square inches, compared with a circular plate of 18 inches, or double that diameter, surface 254 square inches. Here the surfaces are as 1 : 4, whilst the boundaries or circumferences are as 1 : 2.

Charge of 9-inch plate, 6 circular inches, under an intensity of 10°. Charge of 18-inch plate, 12 circular inches, under the same intensity of 10°. Here the charges are as 1 : 2, whilst the surfaces are as 1 : 4; neglecting the difference of boundary, therefore, the charges, as in the preceding experiments, are as the square roots of the surfaces.

On examining the intensities of these plates, they were found to be inversely as the surfaces; thus 6 circular inches upon the 9-inch plate evinced an intensity of 10°, as just stated; 6 circular inches upon the 18-inch plate had only one-fourth the intensity, or 2°·5, being inversely as the surfaces, according to the formula $E = \frac{1}{S}$.

Experiment 13. A circular plate of 9 inches diameter, surface 63·6 square inches, compared with a circular plate of 12·72 inches diameter, surface 127·2 square inches. Here the surfaces are as 1 : 2.

Charge of 9-inch plate (surface 63·6 square inches), 5 circular inches, under an intensity of 8°. Charge of 12·72-inch plate (surface 127·2 square inches), 7 circular inches, under the same intensity

of 8°. The charges here are as 5 : 7, whilst the surfaces are as 1 : 2; that is to say (neglecting the boundaries), the charges are as the square roots of the surfaces.

On examining the intensities of these plates, they were found to be, as in the preceding experiments, inversely as the surfaces.

Experiment 14. Comparison of a sphere of 4·5 inches diameter, surface 63·5 square inches, with a sphere of 9 inches, or double that diameter, surface 254 square inches.

Charge of sphere of 4·5 inches diameter (surface 63·5 square inches), 4 circular inches, under an intensity of 9°. Charge of sphere of 9 inches diameter (surface 254 square inches), 8 circular inches, under the same intensity of 9°. Here the charges are as 1 : 2, whilst the surfaces are as 1 : 4. The charges, therefore, are as the square roots of the surfaces, or as $1 : \sqrt{4}$.

On examining the intensities of these spheres, they were found to be inversely as the surfaces, or very nearly, being as 2°·5 and 9° respectively.

Experiment 15. Circular plate of 9 inches diameter compared with a sphere of the same diameter. Here the actual surfaces are 63·6 square inches for the plate, and 254 square inches for the sphere, being as 1 : 4. We have to observe, however, that one surface of the sphere is closed or shut up; consequently the exposed surfaces, electrically considered, neglecting one-half the surface of the sphere as being closed, are as 1 : 2, and the exposed surface of the plate is exactly one-half the exposed surface of the sphere.

Charge of plate 8 circular inches, under an intensity of 12°. Charge of sphere 11 circular inches, under the same intensity of 12°. The charges, therefore, are as 8 : 11, or as 1 : 1·4, the exposed surfaces being as 1 : 2. The charges, therefore, are as the square roots of the exposed surfaces.

On examining the intensities of the plate and sphere, they were found to be in an inverse ratio of the exposed surfaces, as in the former experiments.

Experiment 16. Comparison of a sphere of 7 inches diameter with a circular plate of 14 inches, or double that diameter. In this case the inner and outer surface of the sphere, taken together, are actually the same as the two surfaces of the plate. The inner surface of the sphere being closed, however, as in the last experiment, the surfaces of the sphere and plate, electrically considered, are therefore not equal, and the surface of the plate is twice the surface of the sphere. The surfaces, therefore, open to external induction are as 2 : 1.

On examining the charges of the plate and sphere, they were found to be as 10 : 14, or as 1 : 1·4, charge of sphere being 10 circular inches, under an intensity of 20°, and charge of plate being 14 circular inches, under the same intensity of 20°. The charge of the sphere, therefore, as compared with the charge of the plate, is as $1 : \sqrt{2}$, that is, as the square roots of the exposed surfaces.

On examining the intensities of the sphere and plate, they were found to be, as in the preceding experiments, in an inverse ratio of

the exposed surfaces. We cannot, therefore, conclude, as already observed (4), that the capacity of the plate is twice that of the sphere.

19. The following experiments are further adduced in support of the preceding :—

Experiment 17. A copper plate 10 inches square, compared with the same plate rolled up into an open hollow cylinder, 10 inches long by 3·2 inches diameter. Here, as in the last experiments, although the surfaces are actually the same, yet, electrically considered, the plate has twice the surface of the cylinder, one surface of the cylinder being shut up.

On examining the charges of the cylinder and plate, they were found to be, as in the preceding experiments, as $1 : \sqrt{2}$; that is, as the square roots of the exposed surfaces, and the intensities in an inverse ratio of the surfaces, which seems to be a general law for closed and open surfaces.

Experiment 18. A hollow copper cube, side 5·7 inches, surface 195, compared with a hollow copper sphere of diameter equal side of cube, surface 103 square inches nearly.

On examining the charges of the sphere and cube, they were found to be as 9 : 10 nearly, charge of the sphere being 9 circular inches, under an intensity of 10°, and charge of cube being 10 circular inches, under the same intensity of 10°. The charges of a cube, and of a sphere whose diameter equals the side of the cube, approach each other, notwithstanding the differences of the surfaces, owing to the six surfaces of the cube not being in a disjointed or separated state.

20. The author observes, in conclusion, that the numerical results of the foregoing experiments, although not in every instance mathematically exact, yet upon the whole were so nearly accordant as to leave no doubt as to the law in operation. It would be in fact, he observes, assuming too much to pretend in such delicate experiments to have arrived at nearer approximations than that of a degree or two of the electrometer, or within quantities less than that of ·25 of a circular inch. If the manipulation, however, be skilfully conducted, and the electrical insulations perfect, it is astonishing how rigidly exact the numerical results generally come out.

GEOLOGICAL SOCIETY.

[Continued from vol. xxviii. p. 562.]

Nov. 23, 1864.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read :—

1. "On the occurrence of Organic Remains in the Laurentian Rocks of Canada." By Sir W. E. Logan, LL.D., F.R.S., F.G.S., Director of the Geological Survey of Canada.

The oldest known rocks of North America, composing the Laurentide Mountains in Canada, and the Adirondacks in the State of New York, have been divided by the Geological Survey of Canada

into two unconformable groups, which have been called the Upper and Lower Laurentian respectively. In both divisions zones of limestone are known to occur, and of them at least three have been ascertained to belong to the Lower Laurentian. From one of these limestone-bands, occurring at the Grand Calumet on the River Ottawa, Mr. J. McCulloch obtained, in 1858, specimens apparently of organic origin, which were exhibited as such by the author in 1859; and other specimens have also been obtained from Grenville and Burgess. These specimens consist of alternating layers of calcareous spar, and a magnesian silicate (either serpentine, white pyroxene, pyrallolite, or Loganite)—the latter minerals, instead of replacing the skeleton of the organic form, really filling up the interspaces of the calcareous fossil, as was discovered by Dr. Dawson, to whose paper, and to that by Mr. Sterry Hunt, Sir William refers for further details.

2. "On the Structure of certain Organic Remains found in the Laurentian Rocks of Canada." By J. W. Dawson, LL.D., F.R.S., F.G.S. With a Note by W. B. Carpenter, M.D., F.R.S., F.G.S.

At the request of Sir Wm. Logan, Dr. Dawson carefully examined the laminated material thought by Sir William to have an organic origin, and he found it to consist of the remains of an organism which grew in large sessile patches, increasing at the surface by the addition of successive layers of chambers separated by calcareous laminæ. Slices examined microscopically showed large irregular chambers with numerous rounded extensions, and bounded by walls of variable thickness, which are studded with septal orifices irregularly disposed; the thicker parts of the walls revealed the existence of bundles of fine branching tubuli. Dr. Dawson therefore concludes that this ancient organism, to which he gave the name of *Eozoön Canadense*, was a Foraminifer allied to *Carpenteria* by its habits of growth, but of more complex structure, as indicated by the complicated systems of tubuli; it attained an enormous size, and, by the aggregation of individuals, assumed the aspect of a coral reef.

In a note, Dr. Carpenter corroborated Dr. Dawson's observations on the structure and affinities of *Eozoön*, but stated also that, as he considered the characters furnished by the intimate structure of the shell to be of primary importance, and the plan of growth to have a very subordinate value, he did not hesitate to express his belief in its affinities to *Nummulina*.

3. "On the Mineralogy of certain Organic Remains found in the Laurentian Rocks of Canada." By T. Sterry Hunt, Esq., M.A., F.R.S., of the Geological Survey of Canada.

Mr. Sterry Hunt first referred to the structure of *Eozoön* as made out by Dr. Dawson, and then stated that the mineral silicates occurring not only in the chambers, cells, and canals left vacant by the disappearance of the animal matter, but in many cases in the tubuli, filling even their smallest ramifications, are a white pyroxene, a pale-green serpentine and pyrallolite, and a dark-green alumino-magnesian mineral which the author referred to Loganite. The

calcareous septa in the last case are dolomitic, but in the other instances are composed of nearly pure carbonate of lime.

The author then gave the results of a chemical analysis of specimens from the different localities, and deduced therefrom the composition and affinities of Loganite; this mineral he considered to be allied to chlorite and to pyrosclerite in composition, but to be distinguished from them by its structure.

In conclusion, the author showed that the various silicates already mentioned were directly deposited in waters in the midst of which the Eozoön was still growing or had only recently perished, and that they penetrated, enclosed, and preserved the structure of the organisms precisely as carbonate of lime might have done; and he cites these and other facts in support of his opinion that these silicated minerals were formed, not by subsequent metamorphism in deeply buried sediments, but by reactions going on at the earth's surface.

XIII. *Intelligence and Miscellaneous Articles.*

REMARKS ON THE LETTER PUBLISHED BY DR. J. DAVY IN THE DECEMBER NUMBER OF THE PHILOSOPHICAL MAGAZINE.

To the Editors of the Philosophical Magazine and Journal.

Collingwood, Hawkhurst, Kent,
December 12, 1864.

GENTLEMEN,

I HAVE received within a few days a copy of Dr. Davy's letter to you, forwarded to me by its author, of whose publication in your Magazine I was till then ignorant (but which bears the date of October 20th, 1864), in reference to certain charges in a late work by Mr. Babbage.

In it he expresses an impression that from what he has learned I would not support Mr. Babbage's statements respecting a certain conversation alleged by him to have taken place at a Council of the Royal Society held on Nov. 23, 1826, between Dr. Wollaston and Sir Humphry Davy—nor indeed respecting a promise alleged to have been given to me by the latter, that Mr. Babbage should become my colleague in the Secretaryship of the Royal Society then vacant and about to be filled up.

From the circumstance of Dr. Davy's sending me a copy of that letter at so considerable an interval after its publication, I cannot but conclude that he is desirous to be confirmed, or otherwise, respecting this his impression; and that, in fact, I am to regard his doing so as a call on me to that effect.

No one can lament more deeply than myself that this subject should have been revived after so long an interval, when both the principal parties concerned in it, and so many of those more or less cognizant of its details while in progress, are deceased. Were it not therefore that I was myself a principal means of inducing Mr. Babbage to allow himself to be mentioned to the President as a candidate, and thereby of causing to him a disappointment which it appears he felt most severely, I should assuredly decline responding to Dr. Davy's appeal. In doing so, however, I shall take care to

confine myself to those precise particulars to which his expressions regarding myself are directed.

And first, as regards the conversation between Dr. Wollaston and the President. In a paper in my own hand-writing, which I can, satisfactorily to myself, identify as having been written either on the 24th or 25th November, 1826, certainly not later, I find it written:

"The President was distinctly asked by Dr. Wollaston whether he intended to use the privilege, by courtesy accorded to him, of naming the Secretary, to which no one would object, or fairly to throw it open to the Council. His answer to the former part of the alternative was susceptible of any sense that one might choose to put upon it; to the latter, it was both in Dr. Wollaston's opinion and my own a negative."

Secondly, as to the question whether the President did or did not promise me that Mr. Babbage should be my colleague. He assuredly never did make to me *that specific promise*, nor, so far as I know, to anyone else. What he *did* promise me (not me only, but others) with regard to Mr. Babbage's pretensions, was that the question of the succession to the Secretaryship, as between him and his competitor, should be referred to the Council; by which I all along understood (as I suppose anyone would) that the relative claims of the candidates on every ground should be fairly taken into consideration at one of its regular morning meetings, and come to be decided on as a matter of free election. Under such circumstances I felt quite confident of Mr. Babbage's success and justified in assuring him that I did so.

In conclusion, it is with the utmost reluctance that I have written the above in connexion with the name of one for whose distinguished talents and services to science I yield to none in admiration, and I entirely appretiate Dr. Davy's motives in writing the letter which has given occasion to this from,

Gentlemen,
Your obedient Servant,
J. F. W. Herschel.

P.S.—The above was written on the date it bears. On consideration, however, I concluded that it would be wrong for me to appear in the matter unless called on by both parties. This is so far now the case that I have received a letter from Mr. Babbage, requesting me to allow him to publish certain extracts from a letter of mine bearing reference to the "promise" or "understanding" above spoken of.

Collingwood, Dec. 20, 1864.

ON THE DISCRIMINATION OF COMPOUNDS OF SESQUIOXIDE OF MANGANESE AND OF PERMANGANIC ACID. BY HOPPE-SEYLER.

The author has discovered in the spectroscope a method of distinguishing the solutions of permanganates from the sesquisalts of manganese, the latter of which have often a similar coloration to the former.

H. Rose, who first described the preparation and properties of

phosphate of sesquioxide of manganese, ascribed the purple colour of the liquid obtained in Crum's method of testing for manganese, by boiling any manganese compound with nitric acid and binoxide of lead, to the formation of sesquinitrate of manganese, and not, as Crum had done, to the formation of permanganic acid.

The author has shown in the most decisive manner that Crum's view is correct.

When a solution, not too dilute, of permanganic acid or its potash salt placed in a glass with parallel sides is brought into the solar spectrum, it produces a very powerful absorption of greenish and yellowish-green light. Solutions of sesquiphosphate of manganese exhibit the same deportment. But if the latter is more and more diluted, the obscurity in the middle of the spectrum gradually disappears *without showing any absorption bands*; while dilute solutions of permanganic acid exhibit *five distinct absorption bands*, of which the first feeble one (starting from red) lies more towards the Fraunhofer's line D, the second dark one is in the middle between C and *b*, the third, equally obscure, lies upon E reaching to *b*, the fourth is between *b* and F, and the fifth and feeblest is in F. These bands are especially distinct when the spectrum is taken on a paper screen.

Sesquichloride and sesquisulphate of manganese show quite a similar deportment to the phosphate, excepting that there are new absorptions in blue and violet.

The liquid, prepared according to Crum's method, shows the five absorption bands in the most distinct manner.—*Zeitschrift für Chemie*, No. 3, 1864.

ON THE CONSTRUCTION OF DOUBLE-SCALE BAROMETERS.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

In a short paper which you did me the favour to insert in your December Number, it was remarked that there was reason to fear that double-scale barometers were not always properly graduated in this country. I was then unaware of the extent to which barometers with incorrect metrical scales have recently been manufactured by the most eminent firms in London. I have in my own possession a standard barometer, two mountain barometers, and a mountain aneroid by four of our first makers, and in each of these the metrical scale is erroneous, while a mountain aneroid by Secretan of Paris is affected by a similar error in the English scale. My friend Mr. Tuckett of Bristol, who has been investigating this subject simultaneously with myself, informs me that all his instruments are likewise inaccurate.

The nature of the error may be explained as follows:—

Suppose a standard brass yard, divided to inches, at the normal temperature of 62° F., to be laid beside a standard brass metre divided to millimetres at the normal temperature of 32° F., with their zero-points coincident. Then by Guyot's Tables for converting inches into millimetres, at their respective normal temperatures, each inch of the English standard will correspond with 25·39954 millims. and 30 inches with 761·986 millims., or 762 millims. very nearly.

Next, suppose the temperature of the standard metre to be increased 30° F., and so to be made equal to the temperature of the English standard.

The coefficient of the linear expansion of brass per degree F. being	0·0000104344
That for 30° will be	0·000313032

Each length of the standard metre which corresponded with the inch of the English scale will have expanded through a space equal to ·00795 millim., and the length which corresponded with 30 inches through ·238 millim.

It is manifest that whatever lengths correspond at the temperature of 62° F. will correspond also at all temperatures common to the two scales.

We have therefore the following relations:—

At the normal temperatures of the standards.	At all common temperatures.
1 inch = 25·39954 millims.	1 inch = 25·3916 millims.
30 inches = 761·986 ,,	30 inches = 761·748 ,,

In a large number of double-scale barometers which have recently been examined by my friends and myself, the metrical reading of 762 millims. has been found to coincide exactly with 30 inches, and therefore to be in excess by nearly a quarter of a millimetre. I accordingly requested one of the makers to inform me on what principle he made the graduation. His answer was as follows:—

"I graduate the English scale with great care by comparison with an English standard; I find from Guyot's Conversion-tables that 30 inches = 762 millims. very nearly; I make a mark on the French side of the scale opposite to 30 inches, and by means of a dividing engine I divide the space between that mark and the zero-point into 762 equal parts, which I call millimetres. I believe this method is universally practised in England."

It is obvious that these millimetres are all too short, and that it is impossible that the reading of a metrical scale so constructed can, when reduced, ever agree with the English one.

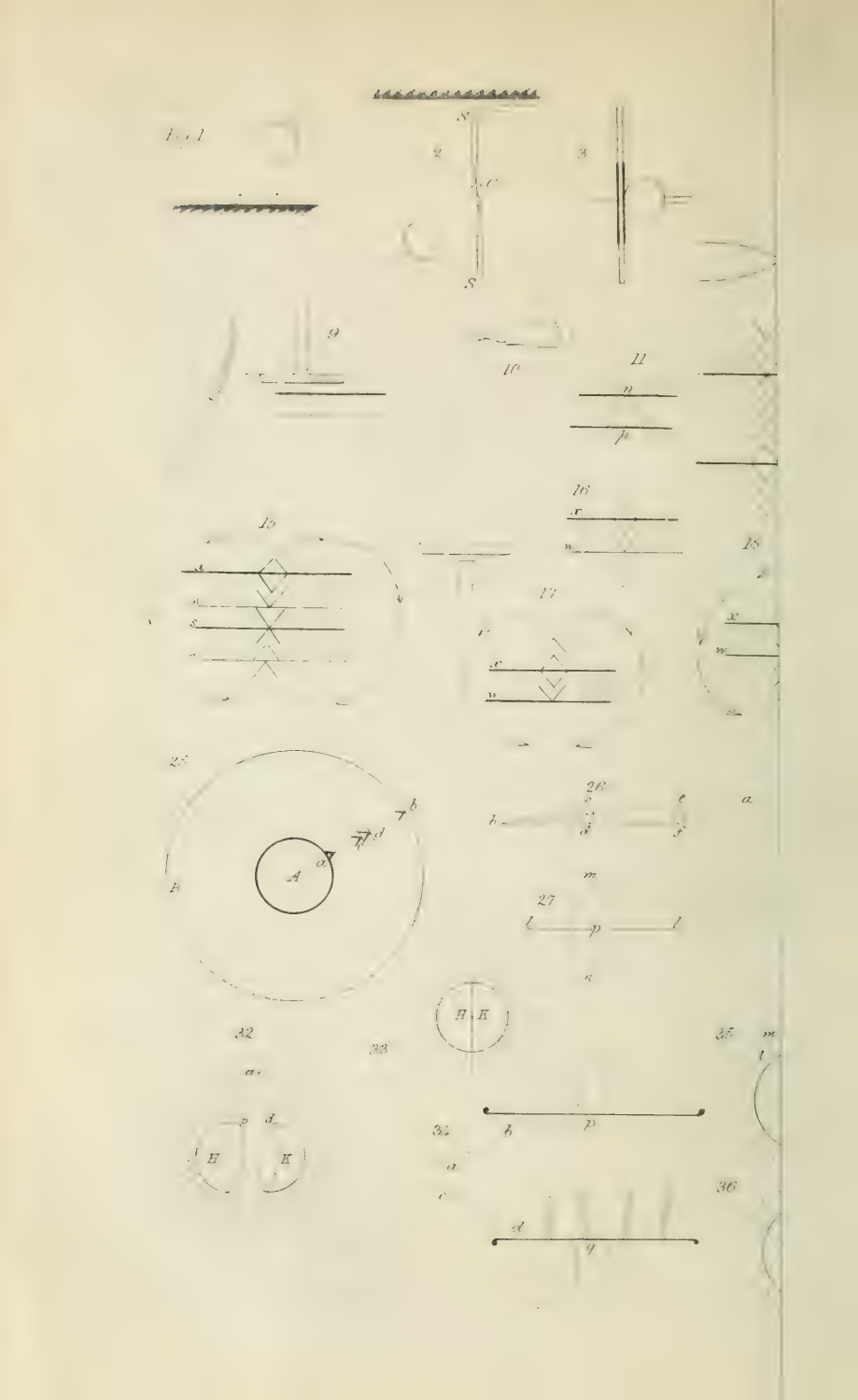
Mr. Tuckett has prepared a new set of conversion-tables, based on the hypothesis of the two scales having the same temperature, and has distributed them to several of the London makers, most of whom immediately recognized the inaccuracy of the method they had been employing, and promised to correct it for the future. It is of course desirable that the production of inaccurate barometers should be stopped as speedily as possible, and I therefore venture to give a word of advice to purchasers. *When a barometer is offered to you, set the vernier to* 30 *inches, and if the metrical reading should be* 762 *millims., reject the instrument at once.* I trust also that the matter will not be lost sight of at Kew and Greenwich, and that barometers with incorrect metrical scales will no longer be passed without comment when sent to those observatories for examination.

I am, Gentlemen,

Your most obedient Servant,

51 Carpenter Road, Edgbaston,
December 12, 1864.

William Mathews, Jun.


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THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

FEBRUARY 1865.

XIV. *An Account of some Electrical Experiments and Inductions.* *By* J. J. Waterston, *Esq.**

[With a Plate.]

IN the various forms of electric discharge and their wondrous diversity of phenomena, can we detect a dynamical condition common to all—a single essential principle upon which the cessation of the excitement depends? Light is a common phenomenon attending discharge, having its origin therein; but throughout nature generally light originates in molecules of ordinary matter: a beginning of the phenomena of light is never unassociated with the material element, unless electric discharge be an exception: yet it may not be an exception, since Fusinieri has proved that metallic molecules are transported from one conductor to the other when an electric spark is made to pass between them. This transport of molecules has always appeared to me to be a phenomenon of the highest interest as respects the essential nature of discharge; and some years ago, while engaged experimenting, I endeavoured to find out if it existed in other forms of discharge besides the spark. On looking over the notes of these experiments lately, it has occurred to me that some of them may perhaps interest cultivators of this branch of science. In the following paper I submit these, along with others made more recently, with the view of investigating certain phenomena of induction on non-conducting matter; also some deductions from Harris's original researches of 1834 and 1839 with respect to the integral of the electric force, and the laws of its distribution in space. The remarks relating to each experiment, appended to the paper, are kept separate in order that the facts

* Communicated by the Author.

may be presented quite unalloyed with matter in any degree open to question or difference of opinion.

Experiments on certain forms of discharge.

1. A disk of sealing-wax, 4 inches in diameter and $\frac{1}{6}$th of an inch thick, held close to the negative excited end of a shell-lac cylinder, then removed and applied to the electroscope, showed no charge; but if before removing it we touch every part of the outer surface of the disk, we find on applying it to the electroscope, that it has acquired a strong positive charge. If we touch only one point, the charge is confined to that point. If we touch with the blunt end of a needle there is no charge; if with the sharp end it is sensible; if with a brass wire it is sensible; with a lead point it is still more sensible, even when that point is held an inch or two from the outer surface of the disk; also with gold-leaf. These phenomena vary with the strength of the excitement of the shell lac, also of the *material* of which the conductor is composed. This last fact is distinctly impressed on the mind after frequent repetition of the experiments.

2. To examine this further, I worked with a jar having about two square feet of coated surface. Its uncoated part was covered with shell lac applied when the glass was hot, and the edges of the tinfoil were covered with sealing-wax, as also the lower part of the jar outside, where the curve was sharp. On the top of the jar was a cake of lac, with a sealing-wax handle to take it off and on; and through its centre passed the charging-rod, which was coated with soft wax, as was also the knob, which was a brass ball 2 inches in diameter. The jar rested on a cake of sulphur which rested on an insulating stool, and was charged by means of another rod and ball uncoated, which was removed after the charge. The soft wax that covered the ball was composed of a mixture of beeswax, resin, and red lead. It was heated and rendered semiliquid before placing on the jar; then, before it hardened, the point of an uninsulated needle was presented about an inch from the knob (first having touched the outside of the jar). Instantly the coated semiliquid surface of the knob opposite the needle was pitted with innumerable small holes, occupying an exactly circular space of three-quarters of an inch diameter. By taking away the needle, touching the outside of the jar, then bringing the needle to its former position, the action could be sustained (until the coating became hard), having the appearance of a shower of particles as from a watering-pan.

The pitted space being then examined by a lens, the appearance was exactly as if a shower of minute projectiles had been forcibly driven through the wax coating to the metallic surface of the

ball. The pitted space was bounded by a perfectly circular ridge, as if there had been a current of air or augmented atmospheric pressure on the part.

In the dark the usual appearance of brush was visible issuing from the needle when it was positive; when negative the star appeared, but the wax coating of the knob was pitted *exactly the same as when positive.*

3. Other experiments of the same kind were made by presenting the point of a penknife to the outer surface of a disk of shell lac upon which there was a thin coating of soft wax in a semiliquid state. The inner surface of the disk touched the wax-coated knob of the charged jar. The effect was very peculiar. When the point was within one-eighth of an inch from the coating of the disk, a sharply-defined perfectly *straight line* of indentation suddenly appeared, forming a small fosse about one-fourth of an inch long and one-fiftieth of an inch broad. This was quickly followed by another of the same kind, radiating from the extremity of the first at an angle of about 60°, and sometimes at right angles; others followed, and sometimes two together, forming the letter T.

The general effect, after the discharging point had been held opposite different parts of the semiliquid wax surface, resembled the marks left by crows' feet on a soft clay surface.

When the rounded extremity of a brass wire was substituted in place of the penknife, a similar effect took place, but with much greater ease, lines being thrown off in rapid succession forming stars. A small brass ball throws off stars.

4. It is only after such lines appear that the disk acquires a charge. When the wax surface has cooled and become solid, no indentation or mark is observed when the wire or knife is presented under otherwise the same circumstances; but charge being still given as before, it seems fair to infer that the particles or molecules of metals are still projected upon the wax surface in lines.

The indented line is not simultaneously impressed.—The end nearest the point of the conductor appears first. This was most distinctly observed when the conductor presented to the molten surface was a needle bent at the point; also a small brass ball half an inch in diameter.

5. With highly-charged jar and knob the following experiments were made with *an insulated circular plate* of tinned iron 4 inches in diameter.

a. Held with its centre facing the *coated* knob at a distance of a quarter of an inch; after a few seconds, removed it, and found no charge.

b. The same process repeated after a very small *hole* had been

made in the coating of the knob (the outside of jar being previously touched); the plate, held as before, obtained a charge of the same electricity as the knob.

c. The plate held with its *edge* to the *completely* coated knob obtained a charge of the same electricity as the knob.

d. The plate held as in *c*, but with *edge covered* with wax, received *no charge*.

In these experiments the insulation of the plate must be very perfect. The only way found to answer was suspending the plate by a flat ribbon of the finest shell lac, heated, and drawn out strong enough, when cool, to withstand the disposition of such plates to turn end on to the acting surface.

6. *e.* A cake of lac was substituted for the metallic plate in 5 *b*, and, like the plate, the cake obtained a charge of same electricity as the knob.

f. Experiment 5 *c* was repeated with the cake, and the effect was a slow gradual charge of electricity different from that of the knob; *i. e.* the knob being positive, the cake becomes slowly and gradually negative.

g. The knob of the jar being touched and the lac cake held with its face close to the coated outside of the jar (which teemed with electricity), it received no immediate charge of same electricity as outside coating, but slowly and gradually it received a charge of the opposite electricity.

h. The same cake held close to the bottom angle outside of jar opposite a part uncovered with wax, receives an immediate charge of same electricity as outer coating.

Note. To remove electrical excitement from shell lac and cakes of non-conducting matter, two small flames were employed, one on each side, simultaneously.

7. The prime conductor of an electrical machine, when hot (cylinder 10 inches in diameter), was coated with shell lac. When cold and the coating hard, the machine was put in action and a blunt wire held near the coated conductor (brass cylinder 15 inches by 3 inches). Brushes were projected from the wire to the coated conductor, and luminous points appeared left by the brush and, as it were, imbedded in the shell lac. To continue to give brushes, it was necessary to remove the wire opposite to parts where these luminous points had not yet appeared.

8. *a.* On taking sparks from this shell-lac-coated prime conductor, each spark makes a hole in the coating; and if we do not hold the ball exactly opposite one of these holes, the electric fire takes an angular course, describing the perpendicular to the surface of the prime conductor, and then along the surface of the lac to the nearest hole (Plate II. fig. 1).

b. A thin piece of orange lac in the shell *s s* (fig. 2), having a

crack in the centre *c*, but the edges of the crack quite close together, is stuck on to the coated conductor, and a spark taken from a brass ball held as shown in fig. 2. The spark traversed the surface of the shell lac and passed through the crack, as shown by the dotted line. The distance is much greater than if the spark had to pass through the air alone without the shell of lac to assist it.

c. A glass plate, when hot, was coated with lac, and equal circles of tin-leaf stuck on the centre of both sides. It was held touching a brass ball projecting from the prime conductor. On working the machine, the plate discharged itself by occasional sparks round the edge of the plate; and when the action of the machine was reduced, a corona of light appeared at the edges of the tinfoil, which fluctuated in breadth and intensity in accordance with the action of the electric machine, which, if kept moderate, did not cause discharge by spark round the edges (fig. 3).

9. In the still atmosphere of a small room, the cap, wire, and gold-leaves of an electroscope were mounted on an insulating stand and charged. Various non-conducting bodies, such as clear amber, lac, heated crystal, &c., were then successively brought close to the leaves in various positions. Attraction was manifested between the excited gold-leaves and the non-conducting body, which of itself was entirely free from excitement, *i. e.* it did not attract uncharged gold-leaf.

a. A stick of clear lac was mounted horizontally on an insulating stand so that its conical extremity was level with the gold-leaves. By gently moving the stand, the pointed surface of lac was brought close to the nearest leaf without touching it. When the distance was about $\frac{1}{20}$th of an inch between extremity of leaf and lac-surface, the force kept the leaf suspended and steady as shown in fig. 4.

b. The charge of electroscope was then changed from positive to negative, and the same point of lac held as before with the same result of attraction—showing that it was not due to excitement pertaining to the lac.

c. The surface of lac brought to touch the leaf; the attraction continued. Removing it gently out of contact, the attraction remained as before.

d. Sometimes a small speck of gold-leaf separates and adheres to the lac; repulsion is then manifested at the point where the speck lies, but attraction continues at the other parts. (See $50\frac{1}{2}$.)

10. *a.* The mechanical effects of the electric explosion were noted between two balls coated with the soft wax. On discharging a Leyden jar between balls thus coated (about $\frac{1}{30}$th of an inch in thickness) and not in contact, *the appearance on each was the same*, viz. a small circular portion of the coating were thrown off and the surface of the ball exposed, forming a perfect

circle $\frac{1}{30}$th of an inch in diameter, and in the exact centre was an excavated cup $\frac{1}{130}$th of an inch in diameter, as if a hole had been drilled to a very minute depth (fig. 5). Examined with a watchmaker's lens and reflected light in several directions, it was a simple concavity with bright surface, as if metallic particles had been torn off. Examined with a microscope magnifying 40 diameters, the cavity was obviously irregular; there was a slightly-marked ridge round the excavated part, and a few minute speckles of brass outside the rim as if thrown off in a molten state.

b. Some experiments were made with the view of discovering what became of the small portion of metal torn off at each discharging surface. When the discharging surfaces consisted of brass balls coated with wax, the appearance was the same in both, even when one ball was much larger than the other. The electric force in the axis of discharge effectually tore apparently the same number of molecules from each surface; but in only one instance was there any appearance of those particles after the explosion. In this case one of the discharging surfaces was pointed lead, the other a wax-coated brass ball. A hole was made in the wax coating as before, and about half an inch from it a small patch of brass, about $\frac{1}{60}$th of an inch in diameter, was found, and in an opposite direction a similar patch of lead. On the other side, at about the same distance from the hole, a small irregular fragment of wax was found adhering, with that peculiarity of surface which showed that only the superficial particles had been in a molten state.

c. When the discharge was repeated by bringing the leaden terminus opposite the hole of the previous explosion, the bright point of the brass ball was found coated with lead; and a grey powder appeared round the hole in the wax coating, spreading round it to a radius of about half an inch. In this case a brush preceded the spark.

11. The mechanical effects of the electric explosion between a pointed wire covered with a thick coating of sealing-wax and a naked brass ball were as follows. The coating gave way about three-quarters of an inch from point, with sound of a smart crack of a whip, and the electric fire passed along surface of wax from the fissure to the ball. The fissure extended completely round the coating, but there was no projectile force given to it (fig. 6). The front part of the glove of wax had completely separated from the hinder, but the distance it had moved in consequence of the explosion was imperceptible.

Description of Electroscope.

12. The electroscope employed in the following experiments is shown in fig. 7, which is drawn to a scale of one quarter.

The cage consists of a brass cylinder, on the top of which is stuck a disk of lac with oval hole in centre, in which was fitted, when soft, an oval plug of shell lac that had been melted upon the wire when heated. The lower end of wire was filed to a knife-edge about $\frac{1}{5}$th of an inch long. This edge, when heated, was brought down on a surface of soft sealing-wax (resin and beeswax), the sides then cleaned, and the knife-edge rubbed on paper so as to leave only the slightest coating of wax. It was then brought down transversely upon the centre of a strip of gold-leaf $1\frac{1}{2}$ inch long and $\frac{1}{10}$th of an inch broad (being less in breadth than knife-edge of wire), lying loosely on gold-beaters' paper. After being trimmed at edges, the angle of divergence is very clearly brought out. It is most convenient to use thick gold-leaf which can be handled. I used the thinnest (what is sold in books and used by gilders) in most of the experiments; but latterly I mounted the electroscope with a leaf ten times as thick, obtained from the gold-beater, and found that there was no sensible difference in the divergence for the same intensity of charge.

Two square openings were cut on the opposite sides of the brass cylinder and covered with thin plates of talc, and on the off side an angular scale was applied, which enabled the divergence of the leaves to be measured roughly, the maximum being about 110°. The insulation was almost perfect; even in rainy weather the leaves would stand at their extreme angle for 10 minutes without sensible diminution.

To obtain some idea of the relation between the intensity of the charge and the angle of divergence, twin brass disks (termed B. D.) were provided (fig. 8), and an insulating handle provided to each, composed of a slender stick of fine hard sealing-wax. If we charge one B. D. inductively from a constant source, such as centre of an excited cake of sulphur (see 35.), then touch it with the second B.D., we divide the charge into two equal parts; then discharge B. D. 2, and touch B. D. 1 again with it, we divide the charge into four parts, and so on; three touchings giving $\frac{1}{8}$, and four $\frac{1}{16}$. The cake of sulphur which served as electrophorus was stuck on the top of a cylinder of sealing-wax, as shown in fig. 20. Another cake of the same with shell-lac handle rested upon it, the two surfaces being in close contact.

As an example of graduation, suppose, after exciting the cake and putting on the cover, we leave it in contact for half an hour; then taking off the cover we apply a B. D. to the centre of excited cake, and touching it with a wire take off an induced charge; and reducing this to $\frac{1}{16}$th by four contacts with the twin B. D., we touch the cap of the electroscope with its edge, and after remo-

ving it observe the divergence of the leaves to be 65°. This is termed *angle of first contact*. We then repeat the process, and again touching the cap, find the angle rises to 75°, a third time to 80°, and so on, converging to a maximum of say 85°, which is termed *angle of last contact*, and is the angle that truly represents the intensity of the electricity of the induced charge, and therefore of the inducing power existing at the centre of the sulphur-excited surface. But from the angle of first contact we may infer the angle of last contact.

Now suppose the limiting angle of last contact due to three contacts of the twin B. D.s, or $\frac{1}{8}$th of the constant charge, is 100°; and of four contacts of the twins, or $\frac{1}{16}$th of the same constant charge, is 85°; and of five contacts, or $\frac{1}{32}$nd of the same, is 60°. We can thus determine the relation between the angle of divergence and the quantity of electricity on the electroscope.

The maximum divergence depends on the proximity of the leaves to the sides of the cage, so it varies with the diameter of the cage and length of the leaves. With cage 6 inches in diameter and upwards the maximum is 70°; and 3 inches in diameter, 105°, the leaves being $\frac{3}{8}$ths of an inch long; but if $\frac{6}{8}$ths, the angle extends to about 115°; and if 1 inch, to 130°: the weight of the gold-leaves seems too small to influence the angle, which seems to be fixed by the repulsion between the leaves themselves and the vertical wire that supports them, and by the attraction of the extremity of the leaves to the sides of the cage. This suggests an arrangement with the wire placed horizontal, as perhaps affording a more equable scale.

Having thus determined the scale of the particular electroscope, it is easy to find its capacity with reference to the B. D. unit.

It must be noted that the electroscope cannot be used as an electrometer when the angle of divergence is near the maximum, because the increment of divergence for the same constant increment of charge becomes very minute towards the maximum. By applying the twin B. D.s to divide and subdivide the charge, we can always keep the angle at a safe distance from the maximum. Beyond a certain intensity, which seems to be a fixed quantity, the gold-leaves discharge towards the air, as it is usually termed (see 1. and 3.). If we hold a small disk of sealing-wax close to the leaves while we bring an excited electric towards the cap of the electroscope, it will not obtain a charge until the maximum is attained.

13. The cap of electroscope in contact with a B. D. took as much off its charge as two of the same B. D.s in contact at the same time with each other and with the first; so its capacity may be valued at 2, that of a B. D. being 1. The following is

an approximate Table of density of charge and angle of divergence :—

20°.	30°.	40°.	55°.	70°.	87°.	100°.	Gold-leaf di harges.
0·10	0·12	0·14	0·16	0·21	0·27	0·40	1·00

Limit of discharging effect of some pointed bodies.

14. On centre of excited sulphur cake placed insulated B. D. with small piece of gold-leaf stuck upon it: touched B. D., then removed it by handle, and while it passed edge of cake a small disk of sealing-wax was held opposite the gold-leaf to receive its discharge (fig. 9). After repeating this three times, the disk was laid upon cap of electroscope, and the leaves diverged 30°.

15. The insulated B. D. with small piece of gold-leaf stuck on rim, was placed on centre of strongly excited sulphur plate; then touched it with a wire and removed it from plate. The charge thus obtained was subdivided to $\frac{1}{8}$, and then applied to cap of electroscope. The same operation was repeated without the gold-leaf. In the first case the angle was 58°, in the second it was 95°, thus showing that the gold-leaf appendage disabled the B. D. from retaining electricity beyond a certain tension. There was discharge from the gold-leaf in the act of leaving the excited plate (14), as was made apparent by placing a small disk of wax close to it at that instant, which, acting as a target for the gold molecules then emitted, showed, when applied to the electroscope, a strong charge of the same electricity as B. D.

16. The same experiment was repeated with silver-leaf, which gave angle 62°; platinum-foil, 50°; tinfoil, 56°; lead-foil, 57°; iron wire $\frac{1}{120}$th of an inch in diameter, 62°. In each the charge was reduced to $\frac{1}{8}$th; so they compare with each other and with gold-leaf 58°.

17. The same experiment was repeated with floss-silk fibre; the charge was less than one-half that carried when gold-leaf was attached, and it charged the wax disk on withdrawal of B. D. same as the metal leaves.

18. The same floss silk, when highly dried on sand-bath, makes no discharge; the angle was 93° with the fibre attached, and the same without it.

19. An extremely fine fibre of shell lac also makes no discharge when one-eighth of the charge amounts to 93°.

20. The following are similar observations taken in rainy weather. The angle was of *first* contact. The charge retained after the B. D. with appendage was removed from excited plate was reduced to one-fourth.

Appendage on B. D.	Angle.
Tinfoil pointed and projecting $\frac{1}{2}$ inch from rim of B. D.	60
" " 1 inch " "	15
Sewing-needle, point projecting $\frac{1}{20}$ inch	70
" " $\frac{3}{4}$ inch	42
" head projecting $\frac{3}{4}$ inch	70
Silver-leaf, projecting $\frac{1}{2}$ inch	44
Silk thread highly dried in sand-bath, the end projecting $\frac{1}{2}$ inch as a bush of extremely fine fibres	100
Without appendage	100

Discharging effect of stroking.

21. When the electroscope was charged to near its maximum, a small disk of fine sealing-wax was placed upon the cap; then the elbow of a bent iron wire with a newly filed surface was gently drawn along the top surface of disk, stroking it as it were. This caused the leaves to descend to 50°, and on taking off the disk from the cap the leaves rose again to their original angle, and the disk was found electrified on the surface stroked. There was no effect by merely touching the disk with the wire at many points.

Gold-leaf, silver-leaf, lead, brass, steel file, &c., employed in a similar way, also reduce the angle to 50°, and not lower.

The stroking of itself does not excite the wax surface when the leaves are at zero.

Note that all metals when *rubbed* on wax disk excite it positively, while silk and woollen excite it negatively. The charge given to the electroscope may be positive in the above experiment, so as to show that the effect is not due to excitement by rubbing.

Discharging influence of flame and incandescent matter, and effect of screens.

22. The insulated B. D. charged to a known degree ($\frac{1}{4}$ charge $=90°$) was maintained in position 5 inches below a gas-flame, the jet being horizontal, and the time measured by metronome. When the flame was reduced to the very smallest possible blue bead, the charge was reduced to $= 40°$. When the flame was the largest possible (No. 2 burner), the reduction of charge was exactly the same in the same time, viz. $90° \times 4$ to $40° \times 1$, or 1·2 to 0·14 density of charge (fig. 10).

23. A bead of gas-flame 2 inches above cap of charged electroscope discharges it until the leaves close to about 1°; certainly not entirely shut, because touching the plate with the point of a penknife brings the leaves sensibly closer.

At the distance of 4 inches the discharge proceeds so far as

to close the leaves to about 5°. At the distance of 6 inches the leaves do not descend below 30°.

24. The insulated B. D. charged $4 \times 90°$ is fixed in position at a distance of 6 inches from cap of electroscope, on a level with it. A drop of burning sulphur on the cap causes the leaves to rise first to a maximum of 69° and then to subside.

25. A waxed thread stuck upon electroscope and lighted, discharged the B. D. as in 24., but did not charge the electroscope until the flame had subsided and given place to a red spark.

26. A match, consisting of a short length of thread steeped in solution of nitre and dried, was stuck on cap of electroscope. The charged B. D., placed same as in 24., opened leaves to 30° before lighting the match; after lighting it they diverged to 60°, not further; and on removing the B. D. the leaves gradually subsided to 20°. Setting B. D. in position again, they gradually rose to 60°.

27. An excited cake of sealing-wax or sulphur held near electroscope so that leaves might diverge to 90°. A red-hot poker was then held about 5 inches above the excited surface of the cake, and the leaves subsided in about three seconds. A low red heat is sufficient to discharge such a surface if the iron is held close to it; but to discharge the excited surface of a cake by a hot iron held close to the opposite side of the cake, the heat must be bright orange-red.

28. If a poker, heated to the extreme possible in a bright fire, is held close to an insulated brass plate charged, no effect of discharge takes place; but if after blowing out the flame of a taper we bring the red incandescent point near the charged plate, it discharges it, but less quickly than if the taper were lighted.

29. *Effect of screens on discharge by flame.—a.* The insulated B. D. charged is placed in position 3 inches from gas bead-flame, and halfway between them an insulated tinned plate, 4 inches in diameter, is held as screen. The charge from 92° is in 15 seconds reduced to 60°. Without the screen the same reduction takes place in a fraction of a second. With insulated screen of wire gauze (sixty divisions to one inch), the same reduction takes place in 15 seconds.

b. The screen becomes charged with electricity different from that on the B. D., but the charge is less than if the screen were acted upon by the free induction of B. D.

c. In such experiments the charge of B. D. is reduced more quickly when it is held opposite the circumference of the screen than when opposite the centre, although in both cases equally hid from the flame.

d. A cake of wax held between gas bead-flame and B. D.,

charged positively, becomes excited negatively *on surface next the flame.*

e. Discharge occurs to the same extent with small screen as without it, but much slower.

f. If the screen is comparatively large, there is little or no discharge if the flame and excited body are on opposite sides and near the centre of the screen; but if near the edge, the discharge goes on the same as if the screen were small.

Analysis of the Magic Pane.

30. The Leyden jar described in 2. was highly charged, *positively inside, negatively outside.* The charging knob, rod, and chain being withdrawn, the jar was placed on a cake of sulphur that rested on an insulating stool. Having touched the outside of the jar, the electroscope was held close to it, and the leaves diverged with negative electricity; the divergence increased as the electroscope was removed outwards, keeping the same level until a maximum was obtained, after which it diminished. It also diminished to zero when held quite close to the outside of the jar. The divergence of leaves was greatest when electroscope was held near the mouth of the jar, and least when held near the bottom.

31. The electroscope being fixed in position within a few inches of the outside of the highly-charged jar, the outside coating of the jar is touched, which causes the leaves to diverge with positive electricity. Then watching them, they are seen gradually to close and then gradually to open again with negative. Touching the outside of jar again, the leaves close and open with positive, then slowly close and open with negative, as before.

32. The same experiment as 31. repeated, except that the mouth of the jar was closed with a cake of sealing-wax. This sealing and closing up of the jar did not prevent the phenomena detailed in 31. from taking place, but whether at a slower rate was not remarked. The jar gradually lost the whole of its charge, although the mouth was completely sealed up.

33. A circular plate of fine red sealing-wax, 6 inches in diameter and $\frac{1}{4}$ inch thick, was coated with circles of tinfoil 3 inches in diameter. It was then placed upon a 6-inch diameter cake of sulphur strongly excited negative (fig. 13). The top coating of wax cake was then touched (fig. 14) and a spark obtained. The wax cake was then removed (fig. 16), and its lower side presented to gas bead-flame (fig. 17). It was then placed with its lower coating on cap of electroscope. The leaves do not diverge until the upper coating is touched, when they spring out with negative (fig. 18). Reversing the cake and bringing the top coating *near*, but not touching, the cap of electroscope, the leaves diverge with faint negative; bringing it down to touch there is no diver-

gence until the other side is touched, when the leaves spring out with positive. Reversing again and bringing lower coating near but not touching cap, faint positive is shown; bringing it down to touch, there is no divergence until upper coating is touched, when the leaves spring out with negative.

This may be repeated many times before the plate is discharged.

34. As these phenomena of the magic pane are fundamentally characteristic of the electric force, and make their appearance in uncoated non-conducting matter after excitement, it is well to have a clear idea of their sequence. To assist in this I submit the following method by diagram.

Let *n* (fig. 11) be an element of a non-conducting surface negatively excited, and *p* an element of another non-conducting surface positively excited. These surfaces may be in contact without discharge or depolarization taking place. The polarized condition is represented by the V lines, their aspect from *p* towards *n* being male or entrant, and their aspect from *n* towards *p* being female or recipient. When the surfaces are in contact, the electricities are masked; their intensity is inappreciably small. On separating these surfaces, mechanical force has to be expended, and electric intensity is generated, or polarized lines are spun having one root or extremity on the positive surface and another on the negative surface. When the surfaces are entirely separated, the electric excitement appears on both sides of each excited surface, as at A and B of *n* (fig. 12), and D and C of *p*. It is proper, therefore, to view an excited surface as having two sides.

In the following diagrams, surfaces that are excited are represented by thicker lines than those that are unexcited.

Fig. 13 represents an element of the coated wax plate resting upon the negative excited surface *s* of sulphur plate. The V lines show the female aspect issuing on both sides of *s*.

Fig. 14 represents the wax plate still resting on the sulphur, but after having its top coating touched. This removes all the V lines above *x*, which is now represented by a thick line, to show that the top surface is now excited or charged; and the aspect of the V in contact with *x* shows that the charge is positive or male; but the charge is masked or engaged by part of the opposite female force at *s*. Part of the force that issues from *s* is free, and is directed in lines that proceed backwards from *s*, and that turn round and, enclosing the engaged lines, appear in front of *x*. This is proved from experiments 30. and 33. In 23. the force that acts inductively on the electroscope is positive, which resides on the interior coating of the jar, so that its influence must radiate in a *curved* line to get at the electroscope.

The characteristic of the magic pane is, having the engaged lines of force enclosed and compressed laterally by the disengaged or free lines, which may issue either from one side or both. Fig. 14 represents them as issuing from one side; but as the free lines, by various causes, are gradually discharged at r surface, the lateral enclosing force of compression diminishes, and the engaged lines expand outwards until on the outer side of x appears the male force in a free or inductively active state: these free lines extend from outside x to outside r, and enclose the engaged lines that extend from inside x to inside r (fig. 15).

Fig. 16. The wax plate in the condition represented in fig. 14 is now shown separate from the excited sulphur cake. The engaged lines become free, and are directed with male aspect from both sides of surface x, and the wax plate seems positively excited on every side.

Fig. 17. The coating on w side has been touched and the lines between w and x are engaged, the charge on w being negative but directed inwards, and masked and enclosed by the free lines issuing from the positive roots on the outer side of surface x.

Fig. 18. The coating on x has now been touched, by which not only the free positive escapes, but also a certain portion that was engaged by the negative that is now rendered free on the outer side of w.

The behaviour of non-conducting matter under conditions of excitement and induction.

35. *The behaviour of an excited electric while isolated.*—A cake of sulphur 4 inches in diameter and $\frac{1}{2}$ an inch thick, after being excited negatively by slapping one surface, α, with a warm silk handkerchief, was placed with this surface resting upon three sticks of sealing-wax set upright (fig. 19). The insulated B. D. was placed on centre of unexcited surface β, then touched with free wire and removed. The induced charge thus obtained was halved by touching with the twin B. D. Then applying it to the cap of the electroscope, the gold-leaves opened to 90° P., showing the inducing force at β to be $90° \times 2$N. (first contact). Other observations were taken from α as well as β, as follows:—

	h m	°		h m	°
From β at	1 24 P.M.	90×2 N.	From β at	1 40 P.M.	80×2 N.
From β at	2 15 P.M.	80×1 N.	From β at	3 2 P.M.	30×1 N.
From β at	11 20 P.M.	85×1 P.	From β at	10 20 A.M.	85×1 P.
From α at	11 20 P.M.	85×1 N.	From α at	10 20 A.M.	85×1 N.

The gradual transition from negative to positive of the electric

forces at the surface opposite to the one excited is a phenomenon that invariably takes place in a few hours after excitement.

36. Two cakes of sulphur, 4 inches in diameter and $\frac{1}{2}$ an inch thick, were luted together at the edges with soft beeswax, then placed upon a strongly excited plate of sulphur of same size that was stuck upon the top of a cylinder of sealing-wax. A brass plate 6 inches in diameter was placed on the top of all and touched (fig. 20). These were allowed to remain in position twenty-four hours. The double plate was then removed and applied to electroscope; the under surface showed negative, the upper positive. These surfaces were then completely discharged by gas bead-flame; and the luting being cut, the interior surfaces of the plates showed no excitement when each was separately applied to electroscope. Again tried with elapsed time twenty minutes, still no excitement.

37. An excited cake of sulphur is placed with excited surface downwards resting on a cake of fine sealing-wax that is stuck on top of a lac cylinder (fig. 21). At first the force at α centre of top surface was $85^\circ \times 8$ negative. It then gradually diminished, until in an hour it became zero. After this it gradually became positive; and when eight hours had elapsed the force was measured, 57° positive, and after seventeen hours 50° positive. The sulphur cake was then taken off, and the force at the centre of its excited surface found to be $95^\circ \times 8$ negative; and at centre of wax cake $95^\circ \times 2$ positive.

38. Two cakes of sulphur were each negatively excited on both sides as equally as possible (fig. 22). They were then laid close together so as to form a double cake, and the outside of this double cake completely discharged by holding a small flame on each side simultaneously. The plates were then separated and reversed, the inside surfaces being now the outside of double plate, which was discharged by two small flames as before. The plates were again separated and examined singly at the electroscope. The sides first discharged were found positive, and the others negative.

39. A negatively excited cake of sulphur 4 inches in diameter, resting on three sticks of wax and with excited side uppermost; on this is placed another cake, *b*, of 2 inches diameter, and on it another, *c*, of 4 inches diameter (fig. 23); *b* takes on slowly but continuously an inductive charge positive. In ten minutes there is no sensible effect; but in two hours it is quite distinct, showing about 50° positive, when adjacent excited surface is $90^\circ \times 4$ negative. The cake *c* is also induced positive. With *b* composed of sealing-wax or beeswax, the effect takes place more quickly than with sulphur.

40. Two 4-inch cakes of sulphur are excited negative on

both their sides, and between them is placed a cake of beeswax. It quickly takes on positive on both sides, which increases in intensity with the time of exposure. A cake of sealing-wax is similarly influenced. A cake of sulphur also, but much slower.

41. A 4-inch cake of sulphur is strongly excited on one surface, α, and then placed upon three upright sticks of sealing-wax. Induced charges were taken from centre of α, and also from centre of opposite side, β. The first was $95° \times 8$ N., the second $80° \times 8$ N. Twenty minutes afterwards the first was $85° \times 8$ N., the second $55° \times 8$ N. A lighted taper was then held opposite α, which reduced its force to zero, and the force at β was then $30° \times 8$ P. Such results could not be obtained from sealing-wax or beeswax-cakes.

42. The α surface of sulphur cake excited as in 41., and lighted taper immediately afterwards held opposite α. The result was zero both at α and β. If the taper, instead of being held opposite α, be held opposite β, the plate becomes a charged magic pane without coatings.

43. A cake of very strongly excited sulphur resting on wax cylinder; upon it are placed three similar unexcited cakes, *b*, *c*, *d*. After ten seconds they were removed and examined separately at the electroscope; *b* was found 60° P., *c* neutral, *d* 20° P.

44. Three cakes of sulphur resting on three sticks of sealing-wax; the lowest excited N. on its upper surface *n* so as to give induced charge from its centre $90° \times 4$. After remaining undisturbed for about forty-eight hours, the centre of *r*, the top surface of upper cake, was found 10° N., and centre of *m*, the lower surface of first or excited cake, 30° P. The three cakes were then separated, and induced charges taken from both sides of each while it rested by itself on the three sticks of sealing-wax (fig. 24). The following was the result:—

Force at centre of *r* greater than at centre of *q* and equal $90° \times 2$P.
Force at centre of *o* greater than at centre of *p* and equal 60° N.
Force at centre of *n* $90° \times 4$ N.
Force at centre of *m* $50° \times 4$ N.

45. A cylinder of fine black sealing-wax 2 inches in diameter and 4 inches high, with two small handles of stick wax projecting at the centre so as to hold it and remove it without touching its surface. It was cleaned with solution of soda, washed with running water, and dried in oven; when still hot placed it on cap of electroscope; the insulated B. D. charged positive was placed on its top surface, and leaves diverged to 80° negative. After three minutes took off B. D., the leaves show 60° negative; then off with cylinder, and leaves fall to zero. Put on cylinder

again but reversed, the leaves diverge to 80° positive. Shell-lac cylinder showed the same effect.

46. A piece of a roll of brimstone 2 inches long, set on end upon the extremity of a cylinder of fine black sealing-wax 2 inches in diameter and 15 inches high, and upon top of the roll a B. D. strongly charged positive. In a few minutes the brimstone was *positive* all over, and the wax *negative*.

The same piece of sulphur was cleaned with soda, rinsed with water, dried in oven, and then set on the wax as before, with charged B. D. on top. In a few minutes it was found to be *negative* all over, and so was the wax.

47. The electroscope charged 98° positive. On its top plate was put a small plate of sealing-wax unexcited. The leaves fell to 90°, which continued steady for some minutes. Then took off the cake of wax, and the leaves stood out again to 98°; and on examining the cake, found that it had acquired a slight negative excitement.

Suspended Dielectrics.

48. White silk thread was for the most part used in suspending them. In its ordinary condition it conducts slowly, as the following experiment shows. (See also 17. and 18.)

a. A brass ball was suspended from a free conductor by a white silk thread 4 feet long. At a distance of about 12 inches below the point of suspension, a brass wire fastened to plate of electroscope pressed against the thread. The ball was now charged, and a few minutes afterwards the gold-leaves began to diverge, and continued to do so slowly. The thread was then heated in a close vessel on sand-bath to about 300° F., then taken out and the ball suspended with it as before. No divergence of the leaves ensued, and for two or three hours this perfect insulating quality was preserved.

b. While the brass ball was suspended from the high-dried thread an excited cylinder of shell lac is brought close below it, held for ten seconds, then withdrawn. The ball has received no charge; but if dust is flying about (as when a hassock is beaten close to the ball), it is soon charged with the same electricity as the shell lac.

c. A brass ball is suspended by the *undried* silk thread, and a negative-excited cylinder of shell lac is held close below it. The ball becomes charged positive, and the thread negative*.

* This appears to have been the case in Dr. Faraday's experiments under the heading "Convection or Carrying Discharge" (Experimental Researches, §§ 1562-67). Being unable to understand the reasoning by which phenomena of apparent induction are represented as due to the con-

d. If the silk is undried and there is dust flying about, the same experiment may be made without any apparent charge to the ball, because the convective charge may neutralize the inductive; but the escape of the negative up the silk line to the electroscope reveals the fact that convection and induction are both at work.

49. Cylinders and plates of sulphur, shell lac, and heated glass coated with melted shell lac were suspended by dried silk lines, and subjected to the inductive influence of an excited rod of shell lac 18 inches long and $1\frac{1}{2}$ inch in diameter. They turned and were attracted like insulated conducting bodies of the same shape, but with less force.

The non-conducting quality of these substances was tested previously to experiment by placing them on the top plate of the electroscope while charged to its full extent, then touching them with free conductor; the gold-leaves, watched closely for some minutes, showed not the slightest change.

50. A ribbon of the finest sort of shell lac was drawn out about 3 feet long, $\frac{1}{10}$ of an inch broad, and $\frac{1}{200}$th of an inch thick. While hanging freely by one end, an excited cylinder of shell lac was brought within a few inches, and attraction was strongly manifested: a thread of dried silk is similarly attracted. A piece of the ribbon was then laid upon plate of electroscope when it was fully charged. A needle or free conductor was then brought down to touch the top surface of the ribbon within about $\frac{1}{100}$th of an inch of the plate. The leaves, watched closely for a minute or two, showed no loss.

$50\frac{1}{2}$. A B. D. charged to $90° \times 4$ was placed with its rounded edge close to the end of a small stick of lac that had just been melted by flame of taper. A small cone immediately rose from the melted surface. (See full-size figure $24\frac{1}{2}$.)

[To be continued.]

XV. *An examination of the applicability of* Mr. Alexander's *formula for the elastic force of Steam, to the elastic force of the vapours of the Liquids as found by the experiments* of M. Regnault. *By* Professor Potter, *A.M.**

TO those who are investigating the development of the mechanical force which arises from the action of the subtile agents causing the phenomena of heat, electricity, magnetism,

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veyance by floating particles, I am inclined to believe that the silk lines employed conducted slowly, as no mention is made of their having been specially dried before making the experiment; and unless they were so they could not prevent an inductive charge.

* Communicated by the Author.

&c. upon dense matter, the law of the elastic force of vapours in contact with the liquids from which they arise, at different temperatures, is a subject of great importance.

Many formulæ have been proposed for the elastic force of steam, which corresponded sufficiently nearly with the observed results through moderate ranges of temperature, but failed completely for great ranges. It is not intended in the present paper to recapitulate these attempts.

M. Regnault has given formulæ for interpolation which enabled him, from his observation through many series of experiments, to construct most valuable tables of the elastic force of the vapours of the liquids in regularly ascending temperatures. These are found in his treatise "Relation des expériences . . . . pour déterminer les principales lois et les données numériques qui entrent dans le calcul des machines à vapeur. Par M. V. Regnault," in two volumes, of which vol. i. is tome xxi., and vol. ii. is tome xxvi. of the Memoirs of the Institute, but the two volumes can be procured as a separate treatise; and from a recent paper in this Magazine they appear not yet to be much studied in this country.

However valuable interpolation formulæ may be for the purposes to which they are applied, yet in physical inquiries we naturally seek for formulæ which are likely to represent the physical laws of the phenomena; which will generally be simpler than the expansions in series with coefficients to be determined from the experiments, which is the constitution of the interpolation formulæ.

In the Number of this Magazine for January 1349 there is an important paper, reprinted from Silliman's American Journal of Science for September 1848, and entitled "On a new Empirical Formula for ascertaining the Tension of Vapour of Water at any Temperature. By J. H. Alexander, Esq." In this paper is a Table occupying four pages, in which the results of the formula are compared with the observations of Regnault, 1844; the Franklin Institute, 1836; the French Academicians, 1829; Taylor, 1822; Arzberger, 1819; Ure, 1818; Dalton, 1801 and 1820; Southern, 1797 and 1803; Bétancourt, 1790; Robison, 1778; and Watt, 1774. Though the formula shows considerable differences from even the more recent experiments in some cases, yet the differences are not more than are found between the experimental results themselves of the different observers at nearly the same temperatures. As the range of temperature is from $-27^{\circ}{\cdot}112$ to $435^{\circ}{\cdot}227$ Fahr., through which the general accordance of the formula with the experiments holds good, we must admit the argument of Mr. Alexander, that his formula accords with the observations of the different

observers as closely as they accord with each other, to be conclusive.

When we consider the formula, we can see that the nature of the experiments must be such that extreme accuracy is unattainable with even the most improved apparatus. When p is the elastic force of the vapour expressed in the pressure of a column of inches of mercury, t the temperature expressed in Fahrenheit's degrees, then Mr. Alexander's formula is $p=\left(\frac{t}{180}+\frac{990}{1695}\right)^6$. The binomial being to the sixth power, a small error in the value of t, either in the graduation of the thermometers or in the observations, produces a considerable change in the value of p; and if both errors existed at the same time and in the same direction, the error in the value of p might become large. We have no need to be surprised that even the results of M. Regnault require this consideration to be kept in mind, and it is the general accordance through long ranges of temperature that we must look for, rather than great accuracy at all points, which the subject is not capable of giving.

Having long ago compared Mr. Alexander's formula with the results for the elastic force of several vapours given at page 298 of Dalton's 'New System of Chemical Philosophy,' part 1st of vol. ii., and found the accordance satisfactory, I have lately undertaken the comparison with M. Regnault's results in the second volume of his *Relation des Expériences*, &c., pp. 374 and forwards. The result of these investigations is contained in the following pages.

To adapt Mr. Alexander's formula for the elastic force of steam to that of the vapours of other liquids, let p be the pressure, let a and b be constants, and t the temperature, then we have

$$p=(a+bt)^6.$$

Now M. Regnault's results give p in columns of millimetres of mercury, and t in Centigrade degrees; so that I shall give the procedure and results in these terms at first, and afterwards show the formulæ for p in inches of mercury, and t in Fahrenheit's degrees.

For *vapour of alcohol*, in the formula $p=(a+bt)^6$, putting t° C. $=0$, at the freezing-point of water M. Regnault found

$$p=12{\cdot}70 \text{ millims.} =a^6;$$
$$\therefore\ a=1{\cdot}527451.$$

Again, at 150° C. he found

$$p=7318{\cdot}40 \text{ millims.} = (1{\cdot}527451+150b)^6;$$
$$\therefore\ b={\cdot}0191920.$$

With these values in the formula $p=(a+bt)^6$ the numbers in the third column of Table I. were calculated; the second column contains M. Regnault's corresponding results, from his Table at page 374 of vol. ii. :—

TABLE I.

Temperature Centigrade by air-thermometer.	Elastic force of vapour of alcohol by interpolation formula.	Calculation by Alexander's formula.
	millims.	millims.
-20°	3·34	2·237
−10	6·47	5·674
0	12·70	12·700
+10	24·23	25·836
20	44·46	48·748
30	78·52	86·556
40	133·69	146·166
50	219·90	236·651
60	350·21	369·665
70	541·15	559·887
80	812·91	825·507
90	1189·30	1188·750
100	1697·55	1676·423
110	2367·64	2320·517
120	3231·73	3158·836
130	4323·00	4235·640
140	5674·59	5602·368
150	7318·40	7318·395

The general accordance of the second and third columns through the range of 170° Centigrade is, I think, sufficient to warrant the conclusion that the elastic force of the vapour of alcohol is represented by the formula $p=(a+bt)^6$; and further research might furnish values of a and b which would produce greater accordance.

TABLE II.

Temperature Centigrade by air-thermometer.	Elastic force of vapour of ether by interpolation formula.	Calculation by Alexander's formula.
	millims.	millims.
-20°	68·90	71·471
−10	114·72	116·964
0	184·39	184·390
+10	286·83	281·500
20	432·78	417·937
30	634·80	605·528
40	907·04	858·585
50	1264·83	1194·238
60	1725·01	1632·774
70	2304·90	2198·009
80	3022·79	2917·681
90	3898·26	3823·840
100	4953·30	4953·296
110	6214·63	6348·050
120	7719·20	8055·800

For the *vapour of ether*, taking the temperatures t° C. $=0$ and t° C. $=100^\circ$ to find the values of a and b in the formula $p=(a+bt)^6$, we find

$$a=2\cdot385739,$$
$$b=\ \cdot01742984,$$

which give the results in the third column of Table II.; and the second column contains M. Regnault's corresponding results, from his Table at page 393, vol. ii.

The general accordance of the second and third columns through a range of 140° C., for so volatile a liquid as ether, is, I think, sufficient to show that the elastic force of its vapour is represented by the formula $p=(a+bt)^6$:

For the *vapour of sulphuret of carbon*, taking the temperatures t° C. $=0$ and t° C. $=100^\circ$, we find the values of a and b in the formula $p=(a+bt)^6$ as follows,

$$a=2\cdot244660,$$
$$b=\ \cdot01618729,$$

which give the results in the third column of Table III., M. Regnault's corresponding results (from his Table at page 402, vol. ii.) being given in the second column

Table III.

Temperature Centigrade by air-thermometer.	Elastic force of vapour of sulphuret of carbon by interpolation formula.	Calculation by Alexander's formula.
	millims.	millims.
−20°	47·30	50·240
−10	79·44	81·634
0	127·91	127·910
+10	198·46	194·245
20	298·03	287·066
30	434·62	414·231
40	617·53	585·230
50	857·07	811·400
60	1164·51	1106·146
70	1552·09	1485·185
80	2032·53	1966·785
90	2619·08	2572·043
100	3325·15	3325·144
110	4164·06	4253·690
120	5148·79	5388·924
130	6291·60	6766·143

The general accordance of the second and third columns is again sufficient to lead to the conclusion that the vapour of sulphuret of carbon has its elastic force represented by the formula $p=(a+bt)^6$.

For the *vapour of hydrochloric ether*, determining the constants a and b in the formula $p=(a+bt)^6$ from the experimental results at the temperatures 0° C. and 100° C., we find

$$a=2{\cdot}783579,$$
$$b=\ {\cdot}01753492,$$

which give the results in the third column of Table IV.; and M. Regnault's corresponding results, from his Table at page 446, vol. ii., are given in the second column.

Table IV.

Temperature Centigrade by air-thermometer.	Elastic force of vapour of hydrochloric ether by interpolation formula.	Calculation by Alexander's formula.
	millims.	millims.
−30	110·24	132·374
−20	187·55	207·360
−10	302·09	314·829
0	465·18	465·180
+10	691·11	671·130
20	996·23	948·035
30	1398·99	1314·236
40	1919·58	1791·426
50	2579·40	2405·017
60	3400·54	3184·566
70	4405·03	4164·196
80	5614·11	5383·030
90	7047·51	6885·700
100	8722·76	8722·754

The general accordance of the second and third columns warrants us in concluding that the elastic force of vapour of hydrochloric ether is expressed by the formula $p=(a+bt)^6$.

M. Regnault's experiments on the elastic force of the *vapour of essential oil of turpentine* extend from 0° C. to 200° C., and by calculating the constants a and b from the pressures at 0° C. and 100° C., the formula does not well accord with the observations; and taking the observations at 50° C. and 150° C. to determine the constants, the results of calculation and observation diverge greatly also; but taking the pressures at the temperatures 100° C. and 200° C. to determine a and b, the formula shows a general accordance with the results of observation, with, however, occasional differences of considerable magnitude. These latter give

$$a=1{\cdot}029373,$$
$$b=\ {\cdot}01224551,$$

with which the results in the third column of Table V. were

calculated; and M. Regnault's corresponding results, from his Table, page 501, vol. ii., are given in the second column.

Table V.

Temperature Centigrade by mercurial thermometer.	Elastic force of vapour of oil of turpentine by interpolation formula.	Calculation by Alexander's formula.
°	millims.	millims.
0	2·07	1·190
10	2·94	2·335
20	4·45	4·281
30	6·87	7·425
40	10·80	12·294
50	16·98	19·574
60	26·46	30·140
70	40·64	45·084
80	61·30	65·750
90	90·61	93·772
100	131·11	131·110
110	185·62	180·092
120	257·21	243·458
130	348·98	324·406
140	464·02	426·640
150	605·20	554·419
160	775·09	712·615
170	975·42	906·760
180	1207·92	1143·114
190	1473·24	1428·722
200	1771·47	1771·470

Though the differences in the above Table are in some cases large, yet I think the general accordance of the second and third columns is sufficient to warrant our concluding that the elastic force of vapour of oil of turpentine is expressed by the formula $p=(a+bt)^6$. This last example shows that it is not safe to take *any two* observed pressures and temperatures for the purpose of determining a and b from the Tables, but the values obtained should be applied to the other temperatures and pressures before we can rely upon them.

As the computations are tedious, I shall not proceed further at present with the numerous vapours examined by M. Regnault, having satisfied myself that the elastic force of the vapours generally is expressed by Mr. Alexander's formula.

For the six liquids of which the elastic forces of the vapours have now been shown to be expressed by the formula $p=(a+bt)^6$, we have the following formulæ for p expressed in inches of mercury and t in Fahrenheit's degrees.

For *steam* or vapour of water (Mr. Alexander's formula in decimals),

$$p=(\cdot 5840708+\cdot 00555555\,t)^6.$$

For vapour of *alcohol*,

$$p=(\cdot 691898+\cdot 0062885\,t)^6.$$

For vapour of *ether*,

$$p = (1{\cdot}210775 + {\cdot}00564785\, t)^6.$$

For vapour of *sulphuret of carbon*,

$$p = (1{\cdot}141374 + {\cdot}00524522\, t)^6.$$

For vapour of *hydrochloric ether*,

$$p = (1{\cdot}441730 + {\cdot}005681901\, t)^6.$$

For vapour of *oil of turpentine*,

$$p = ({\cdot}4734178 + {\cdot}00396796\, t)^6.$$

Fresh researches would probably lead to small modifications of the values of the constants, but I do not think that large changes would be found necessary.

XVI. *On Quartic Curves.* *By* A. Cayley, *F.R.S.**

THE expression 'an oval' is used, in regard to the plane, to denote a closed curve without nodes or cusps; and, in regard to the sphere, it is assumed moreover that the oval is a curve which is not its own opposite, and does not meet the opposite curve†—that is, that the oval is one of a pair of non-intersecting twin ovals. I say that every spherical curve of the fourth order (or spherical quartic) without nodes or cusps may be considered as composed of an oval or ovals lying wholly in one hemisphere (that is, not cutting or touching the bounding circle of the hemisphere), and of the opposite oval or ovals lying wholly in the opposite hemisphere; or, disregarding the opposite curves, that it consists of an oval or ovals lying wholly in one hemisphere. And this being so, the quartic cone having its vertex at the centre of the sphere is met by a plane parallel to that of the bounding circle in a plane quartic curve consisting of an oval or ovals; and thence every plane quartic is either a finite curve consisting of an oval or ovals, or else the projection of such a curve.

Considering first the case of the plane, a line in general meets the oval in an *even* number of points (the number may of course be $=0$); hence as the point of contact of a tangent reckons for two points, the tangent at any point of the oval again intersects the oval in an even number of points (this number may of course be $=0$). The number of points of intersection by the tangent (the point of contact being always excluded) is either evenly

* Communicated by the Author.

† The notions of opposite curves &c. are fully developed in the excellent Memoir of Möbius, "Ueber die Grundformen der Linien der dritter Ordnung." *Abh. der K. Sächs. Ges. zu Leipzig*, vol. i. (1852), to which I have elsewhere frequently referred.

even, and the point is then situate on a *convex* portion of the oval; or it is oddly even, and the point is then situate on a *concave* portion of the oval. Now imagine that the oval is (or is part of) a quartic curve; the number of points of intersection by the tangent is $=0$ or else $=2$; and there is at least one portion of the oval for which the number of intersections is $=0$; for otherwise the oval would be *concave* at every point, which is impossible. Hence there is a tangent which does not meet the oval (except at the point of contact), and we may in the immediate neighbourhood of the tangent draw a line which does not meet the oval at all.

Precisely the same considerations apply to the case of an oval which is part of a spherical quartic, the tangent being of course a great circle; and the conclusion arrived at is that there exists a great circle which does not meet the oval at all; that is, the oval lies wholly in one hemisphere.

I remark that the demonstration would, as it ought to do, fail, if we attempted to apply it to an oval portion of a spherical sextic; the tangent circle meets the oval in a number of points which is $= 0, 2,$ or 4; and the number cannot be for every tangent circle whatever $=2$; but there is nothing to prevent it from being for every tangent circle whatever $=2$ or 4. Hence we cannot, for every spherical sextic, obtain a tangent circle not meeting the oval except at the point of contact; and consequently we do not obtain in the immediate neighbourhood of the tangent a circle which does not meet the oval at all. And in fact such circle does not in every case exist; that is, *the oval portion of a spherical sextic does not in every case lie in a hemisphere.*

It has been shown that the oval portion of a spherical quartic lies in a hemisphere; but we have to consider the case where the quartic consists of two or more ovals. To fix the ideas, let A, A′ be a pair of opposite ovals, and B, B′ another pair of opposite ovals, components of the same spherical quartic. If there exists a tangent circle of A which does not meet B, then there exists in the immediate neighbourhood of the tangent circle a circle which does not meet either A or B; and we may assume that A and B lie on the same side of this circle; for if B were on the side opposite to A, then B′ would be on the same side with A; and we have only, instead of B, to consider the opposite oval B′. Hence we may consider that the ovals A and B lie on the same side of the circle; that is, we have a spherical quartic consisting of or comprising the ovals A and B in the same hemisphere: the two ovals are, it is clear, external each to the other.

But every tangent of A may meet B in two points; consider the whole spherical figure, and supppose that the tangent (or say, the tangent circle) of A, A′ meets the ovals B, B′ in the

points K, L and the opposite points K′, L′: then considering the tangent circle as moving round A, A′ until it returns to its original position, the points K, L, K′, L′ are always four distinct points; and K and some one (say L) of the two points L, L′ will describe the same oval, say the oval B; while the opposite points K′, L′ will describe the opposite oval B′. We have here the oval A included in the oval B (and of course the opposite oval A′ included in the opposite oval B′). But the oval B, *quà* portion of a spherical quartic, lies wholly in one hemisphere; hence the two ovals A, B lie wholly in one hemisphere. It is easy to see that there is not in this case any other portion of the spherical quartic, but that the two ovals A, B are the entire curve.

Reverting to the case where we have in one hemisphere the two ovals A, B external to each other, the spherical quartic may comprise as part of itself another oval C. The ovals A and B, *quà* ovals external to each other, have a common tangent circle (a double tangent of the spherical quartic) which cannot meet the oval C (for if it did we should have six points of intersection); hence in the immediate neighbourhood thereof we have a circle not meeting any one of the ovals A, B, C. We may consider A, B, C as lying on the same side of this circle; for if B were on the opposite side to A, then B′ would be on the same side; and so if C be on the opposite side, then C′ will be on the same side; that is, we have the three ovals A, B, C external to each other, and in the same hemisphere.

There may be a fourth oval, D, and it would be shown in a similar manner that we have then the four ovals A, B, C, D external to each other and in the same hemisphere. But there cannot be a fifth oval, E; the proof is precisely the same as for the theorem *in plano*; viz. taking within each of the five ovals a point, and through these points drawing a conic, the conic would meet each oval in two points, and therefore the plane quartic in ten points, which is impossible.

Passing from the sphere to the plane, the foregoing investigation shows that every plane quartic without nodes or cusps is either a finite curve, or else the projection of a finite curve, of one of the following forms:—

1. a single oval.
2. two ovals external to each other.
3. two ovals, one inside the other.
4. three ovals external to each other.
5, 6. four ovals external to each other.

The last case has been called (5, 6) for the sake of the following subdivision, viz.:—

5. the four ovals are so situate as to be intersected, each in two points, by the same ellipse.

6. they are so situate as not to be intersected by any one ellipse whatever—the distinction being similar to that which exists between four points, which may be either such as to have passing through them as well ellipses as hyperbolas, or else to have passing through them hyperbolas only.

I remark that the limitation of the theorem to the case of a quartic curve without nodes or cusps is necessary, at any rate as regards the nodes. We may in fact find a quartic curve having a single node which is met by every line in at least two real points, and which is therefore not the projection of any finite curve; for if we imagine two hyperbolas so situate that each branch of the one cuts each branch of the other, then it may be seen that there exists a quartic curve approaching everywhere very nearly to the system of two hyperbolas, but having, instead of the four nodes of the system, only a single node, which is such that every line meets it in at least two points.

Cambridge, December 15, 1864.

XVII. *On the condition of the Molecules of Solids.*

By A. G. Girdlestone, *Magdalen College, Oxford**.

THAT gases are bodies whose particles are moving in straight lines is now no hypothesis, but a fact resting on physical demonstration, derived from the phenomena of diffusion, heat, &c. In like manner the motion of the particles of liquids may be demonstrated. The object of the present paper is to give the same certainty to the hypothesis of the molecular motion of solids.

Now there are certain *prima facie* difficulties in the way of such an hypothesis, *e. g.* the cohesion of solids, the fact that they exert no chemical action, that they do not transmit pressure equally in all directions, their inertia, &c., which, however, on further consideration are the most convincing confirmatory evidence that these molecules, equally with those of liquids and gases, are in motion.

For if we adopt the idea that they rotate on their own axes, just as a top does, these phenomena must ensue, as will be seen. Let us imagine first a single top spinning (the gyroscopic top furnishes an apt illustration); this, when in rapid rotation, strongly resists any change of its plane of rotation, and if supposed free from gravity and external forces, would continue to do so. Now if a mass composed of such tops under such conditions be imagined, the axes of rotation lying in every conceivable plane, (*a*) the phenomenon called cohesion would be ob-

* Communicated by the Author.

servable; for an attempt to divide such a mass into two parts would be resisted by the disinclination of the tops in the line of division to be pushed to one side, involving, as this would, a change of their planes of rotation. (*b*) Chemical action could not take place, for there is no jostling of the tops one against another; and therefore if two such masses were placed in contact, the tops in one would not mix with those in the other. (*c*) Nor would the mass transmit pressure equally in all directions, as to do so would manifestly alter their planes of rotation. (*d*) Inertia would result from this state of things, just as cohesion does, from the tendency of the molecules to preserve their respective planes of rotation. The gyroscope illustrates this well, resisting (save in directions parallel to the axis) motion from the hand when taken up, and being superior to gravity so long as in rapid rotation. A system of such tops, therefore, would resist motion in all directions.

Now extending the idea from tops to molecules, and from the comparatively slow motion of the gyroscope to the infinitely rapid motion which we attribute to molecules, these results would increase very greatly in intensity, depending, as they do, on the velocity of rotation much more than on the weight of the moving body; so that the hypothesis in the case of the tops becomes a fact in the case of the molecules, and external forces become so relatively insignificant as to have no effect; and this view is in accordance with the variations of these effects in different bodies, which we may consider as varying with their molecular velocities. If this statement should seem at variance with the effect of gravity and other external forces on matter, it need only be remarked that, under ordinary circumstances, the axes of rotation of the molecules in a body are in every conceivable plane, and therefore their motions tend to neutralize one another's effects in their result as a whole.

At this point, then, only two assumptions concerning matter are made; that it consists of spherical or spheroidal molecules, and that these in solids rotate round their own axes. The former is still only an hypothesis in the case of liquids and gases; it is with the second that this paper deals, with the view of removing it from the region of opinion to that of fact. Hitherto confirmatory evidence alone has been alleged, we may now proceed to more direct proof.

It was stated that under ordinary circumstances the axes of rotation of its molecules being in all planes, a body as a whole does not resist external forces as its molecules do. A crucial test, then, of the theory may be found, if by any means we can cause a parallelism of these axes in a body. We have only to resort to any molecular polarizing force, such as magnetism or

electricity, partially to effect our purpose. Under the influence of these forces, do bodies behave as our theory requires? In Prof. Tyndall's work on Heat*, an interesting experiment is described which shows that a body between the poles of a powerful magnet can be moved only with some difficulty, the resistance experienced being exactly as though it were immersed in a viscous body. Anyone who has handled a revolving gyroscope must be struck with the great similarity of its resistance. The more powerful the magnet, the greater is the effect experienced; and doubtless if we could perfectly polarize a body, it would be impossible to move it save by molecular force. It is to this experiment, described so ably by Prof. Tyndall, that I am indebted for this mode of proof. But there is no need of such costly apparatus to prove the point; the resistance of the keeper of a common magnet, the attractions and repulsions of magnets and electrified bodies are examples of the fact that a polarized body is, while in this condition, comparatively independent of external force in the ratio of its polarization. But we may go further, and ask to what is this polarizing effect due? Each pole of a magnet attracts or repels; consequently the effect of the two on a molecule between them similarly affected by each is that of a couple, causing it to rotate on an axis equatorially situated. Each molecule, then, within the sphere of action rotates in a plane the resultant of its original one and that induced by the magnet. When the magnet is withdrawn, they revert to their own planes. When only a single pole acts, rectilinear motion ensues, and the body is attracted or repelled. With the nature of this directive force we are not now concerned, though it seems probable that magnets are bodies whose atoms are permanently polarized, *i. e.* whose molecular axes are parallel, and which induce the same state in other atoms, just as the pendulums of neighbouring clocks influence one another. But many other facts imperatively demand such an hypothesis as that under consideration—*e. g.*, the heat caused by impact. For how can collisions between molecules be so violent, how can they clash with such mechanical results as they do, unless they are moving with immense velocities so as to cause their momentum to be immense? These effects have been well delineated in the work to which reference has already been made. Indeed all the phenomena of heat prove that the molecules of solids are in motion, while such considerations as their chemical inactivity, and their unequal transmission of pressures, prove that they possess no lateral motion, consequently it must be a motion of rotation round their own axes. A simultaneous vibratory motion of the particles, though of itself insufficient to account for the facts under consideration, is not inconsistent with them.

* Heat considered as a Mode of Motion, p. 35, ed. 1.

The change from solid to liquid and liquid to gas, is admirably represented by a top in its three states of simple rotation, of eddying round in an orbit as well, and of flying off still spinning at a tangent, the taps which cause these changes in a top being, in the case of molecules, represented by application of heat.

It has been assumed throughout that a molecule is spherical or spheroidal, and that a force only when acting has effect on a molecule. The former assumption perhaps scarcely admits of proof, but is in keeping with the fact that the heavenly bodies as well as the smallest natural divisions of organized matter are globular. The latter is a natural view to take; for though the effect of a force is exhibited by a mass after it has ceased to act, *e. g.* of pressure on a plastic body, yet this is owing to the inertia and cohesion of the mass, properties due to molecular motion, as has been shown, and which there is no reason for thinking belong to molecules. Liquids and gases have not inertia, their molecules having lateral movement, and a force has no permanent effect on them, which confirms this view. In addition to these proofs, the mind cannot but be influenced towards the acceptance of such a theory as the above by its simplicity, by its yielding a rational explanation of such phenomena as inertia and cohesion, instead of calling them properties of matter, and by the harmony to which it points between the motion of the minutest portions of matter, and of those grand globes which perform their majestic movements in space.

XVIII. *Note on Vapour-densities.—A Reply to* M. H. Ste.-Claire Deville. *By* J. Alfred Wanklyn*.

In the *Comptes Rendus*, vol. lix. No. 26 (December 26th, 1864), M. Deville renews the discussion of the vapour-density of chloride of ammonium. Having repeated his experiment of bringing together hydrochloric acid and ammonia at the temperature of boiling mercury, he concludes that these gases enter into combination at that temperature with evolution of heat, but not of so much heat as would result if the entire quantity of ammonia and hydrochloric acid had entered into combination. Before examining the account which M. Deville gives of his last experiment, I will make the remark that this conclusion is in itself nowise opposed to the theoretical views respecting vapour-densities entertained by so many eminent chemists.

The doctrine of the correspondence between molecular weight and vapour-density does not require that the vapours given off, on exposing chloride of ammonium to the heat of boiling mer-

* Communicated by the Author.

cury, should consist *wholly* of hydrochloric acid and ammonia in an uncombined state. The mean of the vapour-densities of ammonia and hydrochloric acid being 0·93, whilst the vapour-density of the vapours given off by chloride of ammonium when heated to the boiling-point of mercury has been found to be 1·01 (see Deville and Troost's paper in the *Comptes Rendus,* May 11th, 1863, page 895), it is obvious that these vapours should, strictly in conformity with theory, contain some undecomposed chloride of ammonium. A simple calculation will show that, according to the theory, these vapours should consist of

	By weight.
Chloride of ammonium	17·2
Ammonia and hydrochloric acid . .	82·8
	100·0

On bringing together hydrochloric acid and ammonia at 350° C., we should look for the development of about one-sixth of the heat which would result from complete combination.

Nothing in M. Deville's recent paper indicates in any way that there was more than this. As will be apparent on referring to the paper, he agrees with M. Pebal in considering that there was not the evolution of the total amount of heat which complete combination would produce, but gives no data from which we are able to form a judgment whether or not more than one-sixth of that amount was evolved.

The first point that strikes me when I turn to M. Deville's account of his last experiment, is that the condition that the ammonia and hydrochloric acid should have attained the temperature of boiling mercury before they enter the flask in which they come into contact has not been satisfactorily fulfilled. They were sent through the spiral at far too great a rate. 20 to 25 litres of each gas per hour is nearly 7 cubic centimetres of each gas per *second.* The paper informs us that the length of each tube (which was bent into a spiral) was more than 2 metres, the diameter not being given at all. Unless we suppose a very wide tube to have been taken, or that the length *very much* exceeded 2 metres, each cubic centimetre of gas cannot have remained in the spiral for more than two or four seconds. Is a sojourn of only two or four seconds in this spiral heated to 350° C. sufficient to raise the temperature of a cubic centimetre of gas from 20° C. up to 350° C.? Guarantee that the ammonia and hydrochloric acid had reached the temperature of boiling mercury when they entered the flask, there is none.

The flask in which the gases came together measured from 100 to 200 cubic centims. During each second 14 cubic centims.

of gases entered it; therefore in about ten seconds its contents were renewed. There was no *absolute* thermometer placed in it, the air-thermometer employed for the observation of the rise of temperature on the admission of the two gases being a purely relative one. With gases rushing through it at the rate of 7 to 14 cubic centims. per second, it ceases to be certain whether the temperature of the interior of the flask would be 350°. In short, we neither know the temperature of the flask when the hydrochloric acid was rushing through it, nor when the mixed gases were rushing through it—all that we know being that the latter temperature was sensibly higher than the former.

Turning next to Than's very ingenious modification of the experiment*, it appears to me to be certain that the amount of heat produced by anything approaching to complete combination between the acid and ammonia must have been abundantly indicated by his apparatus. It is quite true that the kind of air-thermometer employed was not calculated to indicate slight alterations of temperature; but a change of much less than 40° C. (the rise of temperature in M. Deville's former experiment) could not possibly have failed to give a decisive indication.

M. Deville's objection that Than's gases were at rest when they came together is without foundation. Inasmuch as Than broke the inner vessel (about the upper third) by striking it violently against the top of his graduated tube, it will be manifest that the gases were in movement when they came together; and inasmuch as the hydrochloric acid was discharged at the top of the tube, the fact of its being of higher specific gravity than the ammonia would be in Than's favour—not against him, as M. Deville argues.

London Institution,
January 12, 1865.

XIX. *On an anomalous Magnetizing of Iron.* *By* Prof. A. von Waltenhofen *of Innsbruck*†.

IN the criticism of the hypotheses of natural science, those facts are especially decisive which not only lend support to one of the conflicting theories, but at the same time take it from another. If the discussion relate to the theory of molecular magnetism as opposed to the theory of the two fluids, there is no lack of facts in favour of the former and in opposition to the latter. To these belong in an especial manner the alterations in the

* *Ann. der Chem. und Pharm.* August 1864.

† Translated by Prof. Wanklyn from the *Berichte der Wiener Akademie der Wessenschaften.*

strength of the magnet due to previous magnetism*, and the relations between torsion and magnetism †.

An experiment of the simplest kind, and which may be regarded as an *experimentum crucis* against the theory of the two fluids, is to be found in my observation of the anomalous magnetizing of iron, which forms the subject of this communication.

In examining the residual magnetism which is displayed by electromagnetic masses of iron after the stoppage of the magnetizing current, I have observed that it is nowise indifferent whether the circuit be broken suddenly or whether the intensity of the current be previously diminished by interposing resistance, and gradually, when it has become nearly zero, altogether stopped.

In the former case the residual magnetism is always much less than in the latter.

I have particularly noticed this difference in thick cores of iron. In such I have not seldom observed that, on the sudden breaking of the current, there was residual magnetism of an opposite character to the temporary magnetism which had disappeared, a phenomenon which may be designated "anomalous magnetizing." These phenomena cannot be looked upon as secondary actions of induced currents, inasmuch as the opening currents are in the same direction with the primary currents, and so, on the contrary, can only go to increase the residual magnetism. As in support of this view I refer particularly to the explanations with which Magnus‡ and Helmholtz§ have discussed the behaviour of electromagnetized masses of iron on breaking the circuit, it does not seem to me to be necessary to dwell upon this part of the question.

I think, moreover, that these phenomena find an explanation equally simple and satisfactory if we abide strictly by the supposition that in the movements of the magnetic particles, besides the tendency to return to the original position of equilibrium, there is also a certain resistance due to friction.

Thus, if we suppose the magnetic particles to be displaced by a magnetizing agency, it is clear, in accordance with the above, that on the gradual diminution and removal of this force they must assume other positions of rest than when it is suddenly removed.

In the former case, in which the molecules are allowed to move slowly, the resistance due to friction prevents a complete return

* Wiedemann, Pogg. *Ann.* vols. c. and cvi.; also Waltenhofen's observations on the Electromagnetism of Steel.

† Matteucci, Wertheim, and Wiedemann. See the "Galvanism and Electromagnetism" of the latter.

‡ Pogg. *Ann.* vol. xlviii. § Ibid. vol. lxxxiii.

to the original position of equilibrium; in the second case, on the other hand, the rapidity of the retrograde movement—in proportion to the greater *vis viva* which the molecules acquire on the sudden removal of the state of tension—carries them still further back towards the original position of equilibrium, and in many instances even overstepping of this original position of equilibrium may occur. When the latter takes place, then the further possibility arises that a number of magnetic particles may remain permanently beyond the original position of equilibrium, and then a magnetic condition of an opposite kind to that set up by the electric current may be produced. A needle the oscillation of which is retarded by considerable friction, will, on the sudden or gradual withdrawal of the magnetic tension, behave so as to render the above description experimentally intelligible.

After these remarks, I will give the numerical data belonging to some of the experiments. In giving numbers for magnetic intensities, a moment of a million absolute units is always taken for the unit.

An absolutely unmagnetic cylinder of the softest iron, 103 millims. long. and 28 millims. in diameter, was magnetized (—) with an increasing intensity of current, so far that its temporary moment reached finally about 60. On suddenly stopping the stream, it showed a residual magnetism —0·20, and on repeatedly making a sudden break in the circuit, it showed a decidedly negative residue. On the other hand, when the magnetizing current was gradually stopped, a regular residue of at least 0·30 always appeared. When the current was again passed in the same direction and suddenly stopped, then the residual magnetism, instead of being 0·30 as on gradually stopping, diminished to about 0, but could not be brought below zero, even on repeating the experiment. But on altering the direction of the magnetizing current and then arresting it suddenly, a very marked anomalous magnetizing showed itself; from which it would seem that a bar of iron in which residual magnetism has been once induced, suffers an anomalous overstepping of the molecular equilibrium easier towards the side on which the molecules have been turned by that former residual magnetism.

As often as the iron cylinder was left undisturbed for several days, lying east and west in a horizontal position, it became again completely unmagnetic, and gave, on repeating the first-mentioned experiment, the anomalous residue —0·20, the temporary magnetism which had disappeared having been 60; it showed, in fine, the same character as when first used. If, on the contrary, the magnetic residue had not been removed by long rest, but had been neutralized by means of opposite cur-

rents, the anomalous magnetizing was not to be produced to the same extent as when the unmagnetic state had come of itself in the manner just described.

It follows from this that the apparently unmagnetic state which is brought about by contrary magnetizing, and which Marianini* has named "latent" or "dissimulated" magnetism, is also in these experiments nowise equivalent to the originally unmagnetic condition.

The same irregularities were displayed by an iron bar of 103 millims. in length and 20 millims. diameter, and which was magnetized in the same spiral up to about 45. The numerical values of the residual magnetism for this bar were not very considerably different from those which the thicker bar had showed at the same intensity of current.

For the rest, both bars were of the softest iron which I was able to get. In conclusion, I have still to remark that in these experiments the magnetizing was effected, as a rule, by means of a gradually increasing stream, and that, every time, the iron core was exposed to the magnetizing influence for several seconds. I have thus taken into account the circumstance first shown by Faraday†, that for the complete development of electromagnetism a measurable time is required‡.

The striking numerical difference in the residual magnetism after sudden and after gradual stoppage of the current is in every instance easy to show; on the other hand, the observation of anomalous magnetism requires undoubtedly delicate experiments; it is also obvious that the appearance of the latter must depend upon the nature of the kind of iron, and so far must be more or less accidental.

Innsbruck, November 2, 1863.

XX. *On the Absorption of Gases by Charcoal.*

By John Hunter, *M.A.*§

In a previous communication I have noticed briefly the results of some preliminary observations on the absorption of gases by different varieties of charcoal. These experiments have been continued, and the present paper contains some additional facts connected with this subject. The same method was adopted as before: the gases, carefully dried, were collected over mercury,

* *Berliner Bericht* (*Fortschritte der Physik*), 1847, p. 488.

† 19, 22, and 23, 'Experimental Researches.'

‡ In Faraday's experiments this time amounted to from some seconds up to perceptibly over a minute, according to circumstances.

§ Communicated by the Author.

and when the volume had been read off, the charcoal was introduced, having been heated out of contact with air and plunged into mercury while still red-hot. The residual volume was observed after a lapse of about twenty-four hours. Of all the charcoals I have examined, that made from the cocoa-nut has by far the greatest absorbing power; its absorption for ammonia being 171·7, and for cyanogen 107·5 times its volume. This charcoal is very dense and brittle, and, when broken, the edges have a semimetallic lustre. The greater number of the experiments were made with this charcoal; its pores are quite invisible, and it absorbs scarcely any mercury during the cooling. The numbers indicating the absorptions in general agree remarkably well, particularly when the absorption is large. In one or two cases, however, there are considerable differences in the several experiments: this is most apparent with oxygen and phosphuretted hydrogen; these gases were most carefully examined, but the results obtained always differed slightly; with oxygen this is probably due to the formation of a variable quantity of carbonic acid, which is readily absorbed by the charcoal. I have determined the absorption for some gases which had not previously been investigated; the principal of these are cyanogen and methylic ether.

The first of the following Tables contains the data from which the amount of absorption was deduced in each case.

V, the observed volume of the gas in cubic centimetres;
D, the difference in the level of the mercury;
P, the barometric pressure in millimetres;
T, the temperature in Centigrade degrees.
C, the volume of the charcoal in cubic centimetres.

The separate experiments are indicated by the numbers 1, 2, 3, &c. The upper line in each contains the volume of the gas before the introduction of the charcoal, together with the observed difference in level, pressure, and temperature; the lower line gives the volume, &c. after the absorption was completed.

Table II. contains the absorptions corresponding to the experiments in Table I. The first column under each charcoal gives the volume of gas, reduced to 0° C. and 760 millims., absorbed by one volume of charcoal in each experiment, and the second column gives the means.

These experiments were conducted in the laboratory of the Queen's College, Belfast.

TABLE I.

		Cocoa-nut.					Logwood.					Vegetable ivory.				
		V.	D.	P.	T.	C.	V.	D.	P.	T.	C.	V.	D.	P.	T.	C.
Ammonia	1	19·75	37·3	771·7	22°2	·044	.....	.....	.....	°	.....	43·40	23·0	746·8	9°3	·203
		11·37	37·0	763·5	17·0		.....	.....	.....	.....	.....	15·62	39·0	744·3	9·0	
	2	33·16	36·5	763·5	17·9	·060	.....	.....	.....	.....	.....	39·62	32·0	744·3	10·2	·158
		21·49	37·0	766·2	16·6		.....	.....	.....	.....	.....	15·62	28·0	742·2	10·2	
	3	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	39·28	35·0	752·5	12·7	·107
		.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	23·52	36·5	759·0	11·4	
Cyanogen	1	37·63	37·2	755·3	6·8	·187										
		14·54	31·0	752·0	8·9											
	2	31·68	40·0	757·0	9·3	·111										
		19·53	34·0	756·8	10·3											
	3	39·84	43·0	751·7	10·3	·080										
		29·42	32·0	748·7	8·5											
Deutoxide of nitrogen		33·11	35·0	770·5	21·0	·082				-						
		25·26	35·5	771·8	22·4											
Chloride of methyle		40·36	36·0	752·5	20·5	·199										
		23·00	34·0	762·4	23·8											
Methylic ether	1	31·68	42·0	752·7	11·2	·072										
		25·08	34·5	756·6	10·4											
	2	40·06	38·0	757·7	11·7	·106	38·73	39·0	766·7	13·3	·195	40·18	45·0	762·3	11·6	·041
		30·81	37·5	762·3	11·5		30·01	40·5	764·2	16·3		36·80	37·0	766·7	12·7	
	3	33·42	32·0	760·0	18·3	·084	40·10	37·3	768·4	18·8	·201					
		26·04	36·5	764·8	16·7		29·51	38·0	767·7	19·1						
	4	43·27	34·5	765·2	17·3	·146										
		32·33	36·0	767·7	17·8											
Olefiant gas	1	40·79	42·0	749·0	18·0	·070										
		34·07	33·0	745·7	15·7											
	2	41·46	38·5	745·7	16·4	·098										
		33·67	34·5	749·4	16·2											
	3	37·41	38·0	767·7	19·5	·073										
		32·95	35·0	767·3	21·9											

Protoxide of nitrogen ...	1	26·26	42·0	756·6	20·3	·099						
		18·27	35·5	760·7	20·1							
	2	26·04	41·0	751·6	19·1	·087						
		18·66	40·3	760·4	19·3							
	3	19·96	39·5	760·4	19·8	·076						
		13·76	34·5	757·9	19·0							
Phosphuretted hydrogen.	1	40·23	40·0	761·6	20·5	·074						
		31·90	38·2	766·2	21·4							
	2	29·34	38·3	766·2	20·3	·092						
		22·57	35·4	759·3	18·3							
	3	29·51	37·0	763·7	19·2	·066	29·42	37·0	772·0	19·0	·167	
		23·87	35·0	761·0	18·8		24·65	36·3	770·8	20·8		
	4	24·99	37·2	757·8	19·3	·082						
		19 75	33·5	764·4	17·3							
	5	25·82	36·0	765·0	17·4	·063						
		21·70	32·5	772·0	18·8							
Carbonic acid	1	39·70	42·5	769·2	16·1	·163						
		26·12	31·0	771·8	14·9							
	2	42·53	42·0	771·8	15·0	·195						
		28·64	29·0	771·0	14·4							
	3	41·20	37·2	771·3	14·7	·195						
		25·74	29·0	761·0	12·4							
	4	38·49	35·0	765·0	12·0	·158						
		28·21	31·0	755·0	12·3							
Carbonic oxide	1	38·22	38·0	767·0	21·0	·112						
		35·76	38·4	768·9	22·7							
	2	29·03	37·3	768·5	20·9	·067						
		25·47	32·5	767·7	19·2							
	3	30·86	34·5	766 2	21·6	·064						
		28·86	32·5	766·2	22·7							
Oxygen	1	39·28	35·0	761·2	10·5	·188						
		35·37	29·0	760·0	11·5							
	2	40·36	41·5	750·0	17·7	·084						
		38·10	34·0	758·8	17·8							
	3	42·10	39·5	759·2	18·2	·093	43·54	36·0	759·7	20·3	·197	
		39·93	35·0	759·3	19·8		39·62	34·7	760·2	17·6		
	4	23·30	38·0	760·4	21·8	·148						
		20·40	34·0	751·6	18·0							
	5	24·65	39·5	757·6	20·1	·101						
		23·09	35·0	752·5	20·1							

Table II.

	Cocoa-nut.		Logwood.		Vegetable ivory.	
	Volume of gas absorbed by one volume of charcoal reduced to 0° C. and 760 millims.		Volume of gas absorbed by one volume of charcoal reduced to 0° C. and 760 millims.		Volume of gas absorbed by one volume of charcoal reduced to 0° C. and 760 millims.	
	Experiment.	Mean.	Experiment.	Mean.	Experiment.	Mean.
Ammonia	171·3 172·1	171·7	.....	.....	128·2 132·7 129·5	130·1
Cyanogen	107·4 107·7 107·6	107·5				
Deutoxide of nitrogen	86·3	86·3				
Chloride of methyle ...	76·4	76·4				
Methylic ether	76·2 77·1 77·0 74·6	76·2	39·1 40·5	39·8	64·7	64·7
Olefiant gas	77·4 67·3 79·3	74·7				
Protoxide of nitrogen	67·5 73·1 71·1	70·5				
Phosphuretted hydrogen	91·8 65·1 74·4 57·5 57·0	69·1	27·5	27·5		
Carbonic acid.....	72·5 62·2 71·1 65·0	67·7				
Carbonic oxide	21·5 21·9 20·2	21·2				
Oxygen	13·6 13·9 21·4 17·0 13·9	17·9	10·07	10·07		

XXI. *Differential Equations of the First Order. Extension of Integrable Forms.* *By* CHARLES JAMES HARGREAVE, *LL.D. Dub., F.R.S.**

LET u and v be functions of x, y, and $\frac{dy}{dx}$ (or p). I propose to designate u and v by the term "correlated functions" when they are so connected that $v' \div u'$ (the accent denoting complete differentiation with regard to the independent variable x) is a function of x, y, and p only, and not of p'.

It is well known that when u and v are correlated functions, the differential equation

$$v = fu, \quad \text{or } \phi(u, v) = 0 \quad . \quad . \quad . \quad . \quad . \quad (1)$$

is soluble by differentiation. This process gives

$$w = f'u, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where w is a function of x, y, and p, and is equivalent to $v' \div u'$. The division of v' by u' has the effect of expelling a differential expression of the second order. If this expression be equated to zero, it is a differential equation of the second order, the solution of which gives the complete primitive of (1); but with this expression we are not here further concerned than simply to observe that it disappears in the formation of (2).

It is an elementary proposition that (2) is the singular solution of (1) when we substitute in it for p its value in terms of x and y obtained by solving (1) with regard to p. It is equally obvious (though I have not seen it noticed) that if we regard (2) as a differential equation proposed for solution, its complete primitive is (1), substituting in it for p its value in terms of x and y derived from the algebraic solution of (2) with regard to p. The function f as derived from f' necessarily introduces the arbitrary constant which is essential in order that (1) may be the complete primitive of (2). All this may be thus shortly expressed:—the eliminant of (1) and (2) with regard to p is the singular solution of (1) and the complete primitive of (2).

It is an immediate consequence of the relation which subsists between u and v, that the equation

$$\phi(u, v, w) = 0$$

(where u and v are any two correlated functions of x, y, and p, and w is $v' \div u'$) is integrable whenever

$$\phi(x, y, p) = 0$$

is integrable.

* Communicated by the Author.

For the proposed equation is simply

$$\phi(u, fu, f'u)=0,$$

which is an obvious transformation of

$$\phi(x, y, p)=0.$$

This theorem has long been known and usefully employed with regard to one particular pair of correlated functions, viz. p and $px-y$, which give $w=x$; and this instance has the peculiarity, that if the substitution be effected twice, we are remitted to the original equation. This will always be the case when u, v, and w, expressed in terms of x, y, and p, are similar in form to the expression of x, y, and p in terms of u, v, and w.

The theorem may be stated more generally in this form:—If the equation

$$\phi(u_m, v_m, w_m)=0$$

be integrable for any pair of correlated functions u_m and v_m, then

$$\phi(u_n, v_n, w_n)=0$$

is also integrable for *every* other known pair of correlated functions u_n and v_n.

If, therefore, we know a few pairs of correlated functions, there is theoretically no limit to the number of integrable forms which may be deduced from a single integrated equation. The substitution of u, v, and w for x, y, and p respectively may be repeated as often as we please, and the results may be varied indefinitely by crossing them with other sets of correlated functions.

A very general process of integration for equations of the first order is that by which we are enabled to solve

$$\phi(x, y, p)=0$$

whenever ϕ is linear with regard to x and y, or linear with regard to y and p. The forms

$$y+xf_1p+f_2p=0,$$
$$p+yf_1x+f_2x=0$$

are always soluble. We may now therefore assert that the forms

$$v+uf_1w+f_2w=0,$$
$$w+vf_1u+f_2u=0$$

are soluble when u and v are correlated functions of x and y, and w is $v'\div u'$.

It will be apparent from the nature of the process, that it cannot be made very useful as a means of integrating any particular equation proposed for solution, as we have no means of

determining *à priori* either the proper soluble form to begin with, or the proper transformation to apply to it. Its utility consists rather in enabling us to make an extensive repertory of integrable forms, which may be consulted in order to ascertain whether any proposed equation lies among them.

The first object must be to find a convenient set of pairs of correlated forms. The more simple they are in form, the more useful they will be likely to be for the purpose of transformation. The following Table contains all that I have met with or discovered of a simple character. The list may be extended indefinitely by combining them one with another; but they soon assume a complex form.

Correlated Functions.

No.	u.	v.	w.
1	x	y	p
2	$ax+py$	$y(a+p^2)^{\frac{1}{2}}$	$p(a+p^2)^{-\frac{1}{2}}$
3	$xp+(a-1)y$	px^a	x^{a-1}
4	$\frac{x}{p}+ay$	$\frac{x}{p}(1+ap^2)^{\frac{1}{2}}$	$(1+ap^2)^{-\frac{1}{2}}$
5	$x+\frac{ay}{p}$	$y\left(\frac{y}{p}\right)^a$	$p\left(\frac{y}{p}\right)^a$
6	p	$\phi p+a\left(x-\frac{y}{p}\right)$	$\phi' p+\frac{ay}{p^2}$
7	$p\left(x+\frac{1}{x}\right)-y$	$\frac{p}{x}(1+x^2)^{\frac{1}{2}}$	$(1+x^2)^{-\frac{1}{2}}$
8	$p^{n-1}+(n-1)ax$	p^n+nay	$\frac{n}{n-1}p$
9	$x^{n-1}+(n-1)ap$	$x^n+na(px-y)$	$\frac{n}{n-1}x$
10	p	$\phi p+a(xp-y)$	$\phi' p+ax$

In this Table we may in each case transpose u and v, and substitute for w its reciprocal, and thus we obtain a new set of correlated functions. Or we may invert each set by finding x, y, and p from the system

$$u=\mathrm{X}, \quad v=\mathrm{Y}, \quad w=\mathrm{P},$$

and considering the new forms so obtained as new sets of correlated functions. Thus No. 2 in the Table produces (restoring

the small letters)

$$u=\frac{1}{a}(x-py),\ v=\frac{y(1-p^2)^{\frac{1}{2}}}{a^{\frac{1}{2}}},\quad w=\frac{a^{\frac{1}{2}}p}{(1-p^2)^{\frac{1}{2}}};$$

and No. 3 produces

$$u=p^{\frac{1}{a-1}},\qquad v=\frac{x-\frac{y}{p}}{a-1},\qquad w=\frac{y}{p^{\frac{a}{a-1}}},$$

or

$$u=p^b,\qquad v=b\left(x-\frac{y}{p}\right),\quad w=yp^{-(1+b)},$$

and so on.

The condition which must subsist between u and v in order that they may be correlated functions is

$$\frac{du}{dx}\frac{dv}{dp}-\frac{du}{dp}\frac{dv}{dx}+p\left(\frac{du}{dy}\frac{dv}{dp}-\frac{du}{dp}\frac{dv}{dy}\right)=0.$$

To apply the process, we may begin with any integrable form, however simple; as, for example,

$$x=fp,\ \text{solved by}\ y=\int f^{-1}x\,dx=\psi x.$$

The Table above given suggests (amongst others) the following cases and solutions:—

Equation.	Solution.
$x+py=f\frac{p}{(1+p^2)^{\frac{1}{2}}}=\lambda p.$	$y(1+p^2)^{\frac{1}{2}}=\psi\left(\frac{p}{(1+p^2)^{\frac{1}{2}}}\right).$
$xp+\overline{a-1}y=f(x^{a-1})=\lambda x.$	$px^a=\psi(x^{a-1}).$
$\frac{x}{p}+ay=f\frac{1}{(1+ap^2)^{\frac{1}{2}}}=\lambda p.$	$\frac{x}{p}(1+ap^2)^{\frac{1}{2}}=\psi\left((1+ap^2)^{-\frac{1}{2}}\right).$
$x+\frac{ay}{p}=f\left(\frac{y^a}{p^{a-1}}\right).$	$\frac{y^{a+1}}{p^a}=\psi\left(\frac{y^a}{p^{a+1}}\right).$
$\frac{y^{a+1}}{p^a}=f\frac{p^{a+1}}{y^a}.$	$x+\frac{ay}{p}=\psi\left(\frac{p^{a+1}}{y^a}\right).$
$\phi p+a\left(x-\frac{y}{p}\right)=f\left(\frac{1}{\phi' p+\frac{ay}{p^2}}\right).$	$p=\psi\left(\frac{1}{\phi' p+\frac{ay}{p^2}}\right).$
$p^{n-1}+n-1ax=f\frac{np}{n-1}=\lambda p.$	$p^n+nay=\psi\left(\frac{np}{n-1}\right).$
$p^n+nay=f\left(\frac{n-1}{np}\right)=\lambda p.$	$p^{n-1}+\overline{n-1}ax=\psi\left(\frac{n-1}{np}\right).$
$\phi p+a(xp-y)=f\left((\phi' p+ax)^{-1}\right).$	$p=\psi\left((\phi' p+ax)^{-1}\right).$

many of which are soluble by ordinary methods. If we apply the first example to

$$x+py=ap^2,$$

ft is $\frac{at^2}{1-t^2}$, $f^{-1}t=\left(\frac{t}{a+t}\right)^{\frac{1}{2}}$, $\psi t=(t^2+at)^{\frac{1}{2}}-\frac{a}{2}\log\left(t+\frac{a}{2}+\sqrt{t^2+at}\right)$;

and the solution is

$$py=t\psi t, \quad t \text{ being } \frac{p}{(1+p^2)^{\frac{1}{2}}},$$

in which we have to substitute for p its two values

$$\frac{y\pm\sqrt{y^2+4ax}}{2a}.$$

To each of these equations, or any case of them, the process may be again applied, and so on to an indefinite extent.

Similarly, if we take $y=xfp$, solved by

$$cx=\psi\left(\frac{y}{x}\right),$$

where

$$\log(\psi t)=\int(f^{-1}t-t)dt,$$

we obtain, almost mechanically, solutions of

$$y(a+p^2)^{\frac{1}{2}}=(ax+py)f\left(p(a+p^2)^{-\frac{1}{2}}\right)=(ax+py)\lambda p,$$

$$\frac{x}{p}(1+ap^2)^{\frac{1}{2}}=\left(\frac{x}{p}+ay\right)f\left((1+ap^2)^{-\frac{1}{2}}\right)=\left(\frac{x}{p}+ay\right)\lambda p,$$

$$y^{a+1}p^{-a}=\left(x+\frac{ay}{p}\right)f(y^ap^{-(a+1)}),$$

$$\phi p+a\left(x-\frac{y}{p}\right)=pf\left(\phi' p+\frac{ay}{p^2}\right),$$

$$p^n+nay=\left(p^{n-1}+(n-1)ax\right)f\left(\frac{np}{n-1}\right),$$

$$\phi p+a(xp-y)=pf(\phi' p+ax),$$

with many other forms; and all other known soluble forms may be extended in a similar manner.

A convenient mode of applying the process is to take any pair of correlated functions u and v; and to endeavour to form combinations of u, v, and w so as to obtain any given form. Thus, taking

$$u=y(1+p^2)^{\frac{1}{2}}, \quad v=x+yp, \quad w=\frac{(1+p^2)^{\frac{1}{2}}}{p},$$

we can solve

$$\frac{x}{yp} = \psi u.$$

For

$$\frac{x}{yp} = \frac{v}{yp} - 1 = \frac{wv}{u} - 1 = \frac{fu \,.\, f'u}{u} - 1.$$

Therefore the equation is solved by $v = fu$, where

$$\frac{fu \,.\, f'u}{u} - 1 = \psi u, \text{ or } (fu)^2 = 2\int u(\psi u + u)\,du.$$

Again, since

$$y(px - y) = vyp - u^2 = \frac{ufu}{f'u} - u^2,$$

the solution of

$$y(px - y) = \psi u$$

is $v = fu$, where

$$fu = ce^{\int \frac{udu}{\psi u + u^2}}.$$

Similarly

$$\frac{px - y}{(1 + p^2)^{\frac{1}{2}}} = \frac{fu}{f'u} - u,$$

therefore $\dfrac{px - y}{(1 + p^2)^{\frac{1}{2}}} = \psi u$ is solved by $v = fu$, where

$$fu = ce^{\int \frac{du}{u + \psi u}}.$$

More generally, since

$$\frac{(1 + p^2)^{\frac{1}{2}}}{p}\phi(x + yp) = f'u\phi(fu),$$

we have for the solution of

$$\frac{(1 + p^2)^{\frac{1}{2}}}{p}\phi(x + yp) = \psi u$$

$v = fu$, where $fu = \phi_1^{-1}(\psi_1 u)$, the suffix denoting integration with regard to u.

Similarly,

$$\frac{y}{p}\phi(x + yp) = \psi u$$

is solved by $v = fu$, where $fu = \phi_1^{-1}(\int u\,\psi u\,du)$.

Take the well-known correlated pair

$$u=p, \quad v=xp-y, \text{ and } w=x.$$

Then

$$px+ay \quad =(1+a)uf'u-afu,$$

$$\frac{(xp-y)^m}{px+ay}=\frac{(fu)^m}{(1+a)uf'u-afu}.$$

Therefore

$$\frac{(xp-y)^m}{px+ay}=\psi u=\psi p$$

is solved by

$$v=fu,$$

where fu is to be derived from the equation

$$\frac{(fu)^m}{(1+a)uf'u-afu}=\psi u;$$

and this is easily found by making

$$fu=u^{\frac{a}{1+a}}\chi u,$$

which gives

$$(1+a)uf'u-afu=(1+a)u^{\frac{1+2a}{1+a}}\chi'u,$$

and

$$\frac{(1+a)u^{\frac{1+2a}{1+a}}\chi'u}{u^{\frac{ma}{1+a}}(\chi u)^m}=\frac{1}{\psi u}, \text{ or } \frac{\chi'u}{(\chi u)^m}=\frac{1}{1+a}\frac{u^{\frac{m(a-2)-1}{a+1}}}{\psi u},$$

which is explicitly integrable.

If we take

$$u=px^{a+1}, \quad v=xp+ay, \quad w=x^{-a},$$

we find

$$\frac{(xp+ay)^m}{xp+by}=\frac{a(fu)_m}{bfu+(a-b)uf'u},$$

so that the more general form

$$(xp+ay)^m=(xp+by)\psi(px^{a+1})$$

is integrable.

Some of the examples which I have given above may be found in an elaborate memoir by M. Malmsten, contained in the seventh volume of the current series of Liouville's Journal. In

the latter part of this memoir the author gives, as a particular case of more general results, the following theorem :—

If $\phi(x, y, p)=0$ be a differential equation, and M be a function of x, y, and p such that

$$(\log \mathrm{M})' = \frac{d\phi}{dy} \div \frac{d\phi}{dp},$$

then

$$\mathrm{M}(dy - p\,dx)$$

is a complete differential; and the integral of $\phi=0$ is obtained by eliminating p between

$$\phi=0,$$

$$\int \mathrm{M}(dy - p\,dx) + c = 0.$$

The value of this theorem is materially affected by a circumstance pointed out by the author, viz. that in many cases the difficulty of effecting the integration of the complete differential is so great, that nothing but the assurance of its being actually integrable would induce one to continue the search for the integral. Numerous examples are given which at once illustrate the method and warrant the remark which I have quoted. On a careful inspection of these examples, I observed that in nearly every instance the equation to be solved was of such a form as to give for its primary solution

$$v = fu,$$

v and u being correlated functions, one of which entered into the original equation, and the other of which was obtained by finding $\int \left(\frac{d\phi}{dy} \div \frac{d\phi}{dp}\right) dx$ when practicable. With the knowledge that u and v are correlated functions, I found it easy to determine the form of f, and thus to escape the difficulty pointed out by M. Malmsten. I prepared a paper having this object in view; but in the course of the investigation the more general results which I have put forward in this paper presented themselves to my mind, and appeared practically to supersede the limited object which I had previously had in view.

12 Fitzwilliam Square, Dublin,
January 5, 1865.

XXII. *Researches on the Mineralogy of South America.* *By* David Forbes, *F.R.S., &c.**

II.

Native Gold.—The valley of the Rio de Tipuani, to the eastward of Sorata, in the department of La Paz, Bolivia, is in all parts extremely auriferous, and contains some of the most productive gold-washings in South America, if not in the whole world.

Since the independence of the Bolivian Republic and the abolition both of negro slavery and the forced labour of the Indian tribes, the greatest difficulty has been in procuring labourers; and in consequence of this scarcity, in combination with the unhealthiness of the climate, the gold-washings† in many parts of the valley, previously worked, have been abandoned, and those still remaining in operation are worked upon a scale far from commensurate with their magnitude and richness.

In 1862, when studying the geology of this part of Bolivia, I examined the valley of the Tipuani River, from its source in the great mountain Illampu (the highest of the Andes of South America) down to the river Mapiri, into which the Tipuani discharges itself, and I now present the results of a chemical examination of the native gold found in the sands of the river at various parts of its course.

Gold from Ancota.—This gold-washing establishment, pertaining to Don Ildefonso Willemil, is situated on the other side of the river, and a little above the village of Tipuani‡; and at present its operations are confined to washing the banks which confine the course of the river, without working the actual bed of the river itself.

These banks, which in ancient times had formed the bed of the river, are composed of rock-débris, coloured deep red from the large amount of sesquioxide of iron which they contain.

The fragments of rocks in them are principally ferruginous clay-slates and greywacke of the Silurian age, with some metamorphic schists and a white granite§, which latter rock appears to

* Communicated by the Author.

† These washings were first known to the Spaniards in the year 1581, but used by the Indians long before.

‡ The name Tipuani comes from "Tipa," the name of the Dragon's-blood tree in the Aymara language, as numbers of these beautiful trees are seen growing on its banks.

§ This granite, composed of white orthoclase felspar, colourless quartz, and black or colourless mica, is the same as mentioned in the former part of this paper as occurring at Illampu; it is there supposed to be of Middle

have been the prime cause of the gold being present. These banks are flushed away by water, and the gold left behind collected.

The gold from these washings is extremely uniform in appearance, possesses a very fine colour, and is considered to be some of the purest of the whole district, and is invariably found in fine scales or plates—very thin, as if beaten out, and from the most minute size up to an eighth or even a quarter of an inch in diameter, but very rarely larger, and has frequently its natural colour heightened by a varnish, as it were, of oxide of iron adhering firmly to it.

Its specific gravity was determined on 210·48 grs. of these scales, and found to be 18·31 at 60° F.

Two analyses were made of this gold. In the first case 14·472 grs. of these scales were dissolved in nitrohydrochloric acid, leaving behind 0·99 gr. chloride of silver, equal to 0·755 gr. silver; and the solution precipitated by oxalic acid gave 13·698 grs. pure gold, after separating which, a little sesquioxide of iron was precipitated by ammonia, and weighed 0·02 gr., equal to 0·013 gr. metallic iron, although it is more than probable that this iron in reality was derived from the ferruginous varnish before alluded to.

In the second analysis 20·03 grs. were used, giving 1·39 gr. chloride of silver, equal to 1·049 metallic silver, and the gold was estimated as loss. These results give the following percentage composition:—

			Average.
Gold	94·64	94·76	94·73
Silver . . .	5·22	5·24	5·23
Iron	0·08	trace	0·04
	99·94	100·00	100·00

No trace of copper was discovered.

Gold from Playa Gritada.—At this place the gold is washed out of the bed of the river itself, which is turned off the one half of its bed by dams; and after extracting the gold, the other half is in its turn treated in a similar manner. The gold, although more or less distributed throughout the whole mass composing the bed of the river, is found in greatest quantity on the rock bed below all the gravel, sand, &c.; and in this locality the firm rock (Silurian clay-slate) is covered by no less than 30 feet of stones, gravel, and sand, which must be removed before the gold is reached.

Silurian age, but a more exact examination of the fossils found in the rocks altered by this granite leads Mr. Salter to think that some of the species may be Upper Silurian, and consequently the granite must be considered to be as late as the Devonian period.

The minerals associated with the native gold are oxide of tin in considerable quantity, black tourmaline, garnet, rounded and highly polished nodules of hæmatite, pseudomorphous crystals of oxide of iron after iron pyrites, a rose-coloured mineral, apparently one of the topaz-group, and some small red fragments, apparently of ruby; metallic tin also occurs, as will be noticed further on. The rock of the bed is generally, if not always, clay-slate, and the loose soil above it is composed of boulders of the granite before described, often of immense size, along with Silurian slates, greywacke, and metamorphic schists of the immediate neighbourhood.

The gold is generally found perfectly free from any rock matrix whatever; and I only succeeded in finding a single and minute specimen which contained any gangue, and in that case it was colourless quartz.

The gold is very regular in size, being best compared in size and shape to melon-seed, and of a very fine colour; occasionally larger nuggets have been found, but more as an exception, the size of a melon-seed being the rule; but comparatively little gold dust is associated with it, and of this most is carried off by the water in the imperfect arrangements for washing which are at present employed.

The specific gravity, taken on 428·49 grs. of the rather larger pieces, was found to be 17·906 at 60° F., whilst some of the smallest pieces, determined separately, gave 17·84 at 60° F. The analysis was conducted as in the last case, and afforded

Gold	93·51
Silver	6·49
	100·00

Gold from Romanplaya.—Still higher up the river, probably some ten miles above the last-mentioned workings at the Playa Gritada, are the gold-washings of Romanplaya, in which the covering of diluvial gravel &c. was found to be fully 60 feet in depth above the solid rock on which the gold lies, and appears to be composed of the same rocks as at the Playa Gritada.

The gold in appearance much resembled the last, and its specific gravity, taken at 60° F., using an amount of 150·69 grs. in particles of about the size of a small melon-seed, was found to be 18·672.

On analysis, its percentage composition was found to be:—

Gold . . .	94·189
Silver . . .	5·811
	100·000

Gold from the Head of the Valley of Tipuani.—Still higher up, and in the small streamlets which, when united, form the river of Tipuani, gold-washings are carried on by parties of one or more men, whose success is extremely variable, but who probably make, under ordinary circumstances, at least some two dollars a day, independent of the lucky chance of hitting upon some larger auriferous deposit. The gold in such washings, as might be expected from being nearer its source, is not so pure as lower down, and also does not present that smooth or flattened appearance so characteristic of the gold derived from the washings in the main river, and from which it is at a glance distinguished. It resembles far more the product washed out of gold quartz or veinstone after crushing. In fact it has not been as yet pounded, flattened, and smoothed by the constant action of the pebbles of the river into which it sooner or later might expect to be carried down.

Even the smallest particles of such gold, when dissolved, generally show quartz or other gangue remaining insoluble; and for this reason, as well as from minute air-cavities which the irregularity of form much favour, the specific gravity is generally lower, and in the case of the specimen here reported was found to be 16·07 at 60° F., although small pieces were employed as free from impurity as could be procured. Some of the pieces presented distinct traces of monometric crystallization, the edge of the octahedron being visible on several specimens.

The analysis was made as in the former case, only with the exception that the amount of insoluble, which when seen under the microscope appeared to be nearly altogether quartz, was determined by dissolving the chloride of silver obtained in the course of analysis in caustic ammonia, and afterwards deducting the insoluble residue from the previously-found amount of chloride. The following percentage results were obtained:—

Gold	91·96
Silver	7·47
Iron	traces
Insoluble	0·57
	100·00

The various specimens of gold from the valley of Tipuani here examined will consequently give the following results:—

	Head of valley.	Romanplaya.	Playa Gritada.	Ancota.
Gold . . .	91·96	94·189	93·51	94·73
Silver . .	7·47	5·811	6·49	5·23
Iron . .	trace	....	....	0·04
Matrix . .	0·57			
	100·00	100·000	100·00	100·00

As a rule I have found by observation that the gold is purer in proportion as it is further from its source, provided always that, in making this comparison, equality of size of the particles is also attended to; for since the larger particles cannot be so much acted upon, we naturally find them in general less pure than the smaller scales associated with them in the same locality.

Tin.—The occurrence of this metal in its metallic condition in nature is doubtful; and although it has been reported as occurring in several parts of the world, yet the evidence is on the whole not altogether satisfactory or conclusive, and I propose giving here an account of my examination of the cases in which native tin has been reported in Bolivia.

In the year 1859, Mr. Falkenheimer of La Paz showed me two rather large pieces of metallic tin, enclosing (as if from having been poured out over or amongst it) rocky matter; these had been found at Oruro in Bolivia, and there was no reason to doubt the accuracy of the particulars of the occurrence. The tin was found nearly pure on examination, and externally covered with a coating of oxide. I discovered, however, that the sands of the river on the banks of which it was found contained abundance of Cassiterite, and preferred explaining its occurrence as probably due to some of this Cassiterite having been reduced on occasions of forests being on fire; but as such fires not unfrequently have been known to arise from a stroke of lightning, and so were not the effect of human agency, it might be a point for discussion whether tin so produced could be entitled to bear the name of native tin, and rank as a mineral*.

Subsequently, however, Mr. Philip Kroeber sent me, from the gold-washings of the Playa Gritada, specimens of metallic tin, which he informed me were invariably found associated with the gold left in the washing-apparatus (lavadero); and I resolved on my visit to that locality in 1862 to examine carefully into the matter. No tin ores were known to occur there when I arrived †, but upon inquiry I found that since the year 1846 rosary pumps had been introduced to keep the water out of the excavations, and that the beads of these pumps were in later years formed of spheres of cast tin brought some three or four hundred miles from Oruro. I felt at first quite disposed to attribute the tin to some accident connected with this source.

I must admit that a closer examination of the facts of the

* The argument for considering it a mineral would apparently be quite as strong as in the case of Struvite, which was first found in the foundations of the church of St. Nicolas at Hamburg.

† I found them (Cassiterites) in abundance, however; but, from ignorance, they had invariably been thrown away, not having even been known to contain tin.

case did not appear to confirm this view. The tin which was used for the pumps, when brought such a distance, was quite dear enough to cause the proprietors not to be prodigal of it; and the quantity found in the washings appeared to be too great to attribute it to this cause, especially as all the pieces found in the washing-apparatus were infinitely larger than could arise from the mere abrasion of the pump-heads. Added to this, it must be remembered not only that the ground in which the pieces were found was virgin, but that the tin itself came from the gold stratum, fully 30 feet below the surface. In order to judge better, I resolved to examine the coarser and heavier particles of mineral left behind in the lavadero at the conclusion of the operation of washing the gold, and in consequence obtained about four or five pounds' weight of this residue. After separating a number of pieces of steel and iron, which from their shape could easily be recognized as fragments of the work-tools, I found numbers of nuggets and irregular shots of metallic tin, one piece being above one ounce in weight, and, to my astonishment, along with these an abundance of Cassiterite, which formed nearly the whole of the residue, although its existence had not before been even suspected.

Passing the whole quantity through a sieve with fifty holes to the square inch, I separated it into two parts: the finer, weighing about 2 lbs., consisted chiefly of Cassiterite, and amongst it many shots and grains of metallic tin; but the larger, weighing 15,109 grs., was sufficiently coarse to enable me to pick out its separate constituents and weigh them separately, resulting in the following mechanical analysis:—

Cassiterite, more or less pure . . .	11,115	grains.
Red hæmatite, in polished nodules . .	1,368	,,
Pseudomorphic $F^2 O^3$ after iron pyrites.	110	,,
Black tourmaline, in fragments . . .	214	,,
Garnet, red, in crystals	113	,,
Andalusite (or topaz?)	112	,,
Metallic tin	1655	,,
Undeterminate rock	422	,,
Total	15,109	,,

The largest piece of tin found weighed 505 grs. The relative proportion of the tin present seemed so large, that I had the curiosity to weigh the fragments of iron work-tools found along with the mass, and found them in all to weigh only 1069 grs., although two of the pieces exceeded in size the largest piece of tin; and from this circumstance one can but infer the improbability of the tin having been derived from the pumps as I had so determinedly endeavoured to prove, notwithstanding

that the proprietors and managers of the speculation quite ridiculed the idea of such being the case. Are we to suppose that some of the Cassiterite may have been reduced and melted by forests on fire, as before hinted as possible although not very probable? I record the facts here, but must say I am puzzled to come to a definite conclusion; for the tin itself in no specimen yet observed was associated with a veritable rock matrix. True it is that it was always extremely impure, and contained stony matter throughout its substance, as if entangled in it, as would be expected in case it had been thrown on to the ground when in a fused state; yet its appearance left the distinct impression on the mind of its having been melted in an ordinary manner.

The specific gravity taken at 60° F. on 556·8 grs. of the smaller and purest fragments was found to be 7·502, which, being considerably higher than pure metallic tin, made a chemical examination of the tin desirable; and its analysis was conducted as follows.

On attempting to dissolve it in nitric acid, I found it so passive, that even several hours' boiling in the acid made no impression whatever on it. I found, however, by heating the tin up to near its melting-point, and allowing it to cool slowly previously to immersion in the acid, that it then was readily acted upon with formation of oxide of tin.

26·74 grs. thus treated afforded 27·12 of tin along with insoluble residue and arsenic acid, and, deducting these, 26·75 binoxide of tin, equal to 21·06 metallic tin, or 78·75 per cent. tin.

The nitric-acid solution was, after neutralization with ammonia, treated with hydrosulphate of ammonia, and left sulphide of lead, with a little sulphide of iron and copper, which afforded respectively 7·43 grs. sulphate of lead, equal to 5·46 grs. metallic lead, or 20·42 per cent., a trace of copper, and 0·08 gr. sesquioxide of iron, equal to 0·05 iron, or 0·20 per cent.

Arsenic was determined on a separate portion (24·08 grs.) by dissolving in hydrochloric acid, passing the evolved gas through nitric acid, and determining the arsenic acid as arseniate of ammonia and magnesia, 0·11 gr. being equivalent to 0·04 metallic arsenic, or 0·17 per cent. The rocky insoluble matter was also separately determined on a fresh portion of the tin. The results thus obtained, when tabulated, will stand as follows:—

	in 26·74.	in 100.
Tin	21·06	78·75
Lead	5·46	20·42
Copper	trace	trace
Iron	0·05	0·20
Arsenic	0·04	0·17
Insoluble gangue	0·30	1·12
	26·91	100·66

A second analysis gave the following confirmatory results:—

Tin	79·52
Lead	19·71
Copper	0·09
Iron	0·19
Arsenic	trace
Insoluble gangue .	0·49
	100·00

XXIII. *Further Statements concerning the History of Calcescence.* *By* Dr. C. K. Akin*.

THE last Number of the Philosophical Magazine contains an article "On the History of Negative Fluorescence," by Prof. Tyndall, to which I intend herewith to reply. As this is a personal discussion, of course I shall have to allude to personal matters; but, in doing so, I shall endeavour not to imitate the language adopted by Prof. Tyndall towards myself, more or less, throughout his paper. In stating this, however, I do not wish to complain of Prof. Tyndall; for, besides that language of this kind always recoils upon the person who uses it, he has adopted the same tone in his discussions with Professors Tait and Thomson, and in such excellent company I can very well bear Prof. Tyndall's contumely.

1. It is now probably from eight to ten years ago that, as a student at the University, and the notions which I was then being taught regarding the nature of heat and radiations being rather vague and inaccurate, I proposed to myself the "conversion of heat into light" as a problem to be solved in the course of my future scientific career. In the year 1860 I attended the Meeting of German naturalists at Königsberg, at which Professor Knoblauch communicated some new researches of his on the interference of Herschellic rays. I then observed to one of the Königsberg Professors how much more simple experiments of this kind would be if it were possible to convert Herschellic rays into visible or Newtonic rays, in a similar manner to that in which Prof. Stokes had shown how to convert Ritteric rays into Newtonic rays. In February 1862 I made a stay at Cambridge, when I had the advantage of seeing the principal experiments on fluorescence performed by Prof. Stokes himself. Having then in my possession a little German treatise which contained a short account of all that had been published at that period on the subject of fluorescence, I began to peruse it; and in the course of that perusal the main ideas which I have since published on the

* Communicated by the Author.

transmutation of rays occurred to me*. I immediately communicated on the subject with Prof. Stokes, hoping that he would enable me to bring my speculations to the test of experiment. Prof. Stokes expressed himself in very high terms on the importance of the subject—terms which I have since often repeated to others, and which have frequently encouraged me to persevere when my patience was nearly exhausted; and when, towards midsummer 1862, I left Cambridge, he was good enough to express his regret that it had not been in his power to give me an opportunity of making the experiments I had proposed. In November of the same year, *i. e.* 1862, I went to reside at Oxford, and so much was my mind engrossed by the matter in hand, that the very first time I met Mr. Griffith, then, as now, Deputy Professor of Experimental Philosophy in the University, I broached the subject to him, when I was greatly pleased to find that the matter interested him, and that he was ready to make experiments on it with me. Consequently, in the month in question, or early in the December following, we made some trials with the oxyhydrogen jet; and as these gave no satisfactory result, we began to make experiments on the sun a short time after. These latter experiments were continued till June 1863, when I drew up a paper on the subject, which I forwarded to the Royal Society. Being called by urgent private matters to town at that time, it was arranged that, during the week or ten days intervening, the experiments, which appeared then to be in a very forward state, should be completed, so that an account of the result might be presented by me to the Royal Society at the last Meeting of the then session. The expectation founded on this arrangement, however, was not fulfilled, and I was consequently advised to withdraw also the introductory papers already forwarded for communication to the Royal Society. Although, in consequence of private circumstances

* I may as well state now that the article on fluorescence in Cornelius and Marbach's *Physikal. Lexicon* (the work being one intended for popular circulation and for reference, not for perusal, became first known to me from M. Emsmann's paper on "Negative Fluorescence," published in Poggendorff's *Annalen* in 1861. It so happened that, at the time when this last-mentioned paper appeared, I was staying at Paris, where I had no access to Poggendorff's *Annalen*; and when in the following spring I had an opportunity of seeing the *Annalen* at the University Library at Cambridge, the volume in which M. Emsmann's paper was published was with the binder; so that the paper became actually known to me only in the winter of 1862-63 at Oxford. In my paper in the Reports of the British Association, in which I have duly adverted to M. Emsmann's publications, I did not think it necessary to mention these facts, as they were of no scientific, but only of personal interest. For an appreciation of the contents of M. Emsmann's paper I must refer for the present to the historical Appendix in the Reports of the British Association for 1863, p. 99.

which then arose, I could continue to give my time and attention to scientific researches only at a great sacrifice, I yet returned towards the end of June to Oxford, in order to complete the experiments, if possible, in time for the Meeting of the British Association, which was to assemble towards the end of the August ensuing at Newcastle; yet, although my return originated in previous arrangements, for reasons which have never been explained to me, no opportunity was given me after my return to proceed with the experiments. The suspense which I then underwent, my mind being full yet of other pressing anxieties, has left an indelible mark of pain on my memory. This, however, is not now the question; I will therefore merely state that, although greatly depressed and discouraged in mind, I attended in August the Meeting of the British Association; that I read the papers which I had prepared before the Mathematical and Physical Section; and that the Committee of the Section, in consequence, resolved to recommend the principal paper I had read for publication *in extenso* among the Reports of the Association. The Association further resolved to appoint Mr. Griffith and myself a committee to carry out the experiments proposed by me, for which purpose a special grant was placed at our disposal. In consequence of this latter resolution, I again took up my residence at Oxford in the spring of 1864, although at some sacrifice to my feelings; but the proceedings which I had experienced in the summer of 1863, I am sorry to say, were soon recommenced, and my plans were again defeated. Instead of a report, I was thus obliged to forward to the President of the Mathematical and Physical Section of the British Association, for the Meeting at Bath in September 1864, a letter of which the following is the substance:—"I am sorry to have to state that, owing to the scarcity of clear sunshine at Oxford last summer, and to Mr. Griffith being mostly unable to give his time to our work when the weather happened to be favourable to it, the experiments intended to have been made were left in August unfinished. I had the greater reason to feel disappointed at such a conclusion, as the methods and apparatus devised for the purpose appeared, from preliminary trials, competent to effect, in part at least, the proposed object, and as our preparations for the more decisive experiments were in a very forward state."

2. Meanwhile, in the abstract of a lecture delivered by Prof. Tyndall at the Royal Institution, and which was published in the 'Reader' in March 1864, a sentence had attracted my attention which will presently be mentioned. Being then on the point of writing a letter to the editor of the 'Reader' on a different subject, I took occasion to add the following postscript, which, together with the letter to which it was appended, appeared in

the 'Reader' of April 2, 1864:—"I profit by this opportunity to call your attention to your report of Prof. Tyndall's late lecture at the Royal Institution. I suppose it was only by inadvertence that you said, 'such a change of period [or of invisible, less refrangible, into visible rays], *Prof. Tyndall believes*, occurs when a platinum wire is heated to whiteness by a hydrogen flame,' &c.; for in your own columns of September 26, 1863, that explanation, communicated by me for the first time to the British Association last year at Newcastle, was published in my name,—a fact which I cannot but believe Prof. Tyndall has referred to." As an afterthought, and as an act of courtesy to Prof. Tyndall, I wrote to him, a day or two after, a private note, enclosing a set of proof-sheets of my paper on Ray-transmutation, then printing in the Reports of the British Association. In answer to that note, I received a letter from Prof. Tyndall, dated from the Isle of Wight, in which he observed on the "singular coincidence of thought" that had arisen between us, and stated that "the piece of work which he had set before him for attack on his return" to town was a series of experiments on "that very subject" that engaged my own attention. I was somewhat astonished at this latter statement; for, Prof. Tyndall having read, as he himself informed me, the article on "Calcescence" in the 'Saturday Review,' was aware that the British Association had confided to Mr. Griffith and myself the task of executing the experiments, for which I had submitted the plans. As to the "coincidence," real or supposititious, between Prof. Tyndall's "thoughts" and mine, it could extend only to the explanation of the origin of lime-light; and Prof. Tyndall neither then nor has he since ever stated that, before reading of my own method in the 'Saturday Review,' he had planned any experiments on calcescence similar to those I had submitted to the British Association. Others, before both myself and Prof. Tyndall, had thought that they had recognized phenomena evincing "negative fluorescence," and yet were unable to devise, for the purpose of proving its existence, methods analogous to those adopted in the production of fluorescence. That Prof. Tyndall had guessed for himself, or perhaps but remembered, the explanation of lime-light, could form no right on his part to interfere with the experiments I had devised, and was then engaged in working out by authority of the British Association. However, Prof. Tyndall's letter being on the whole written in a friendly and courteous tone, and knowing the value of a conciliatory spirit, I went to see him a few days after, by appointment, at the Royal Institution. Prof. Tyndall then spontaneously stated to me that he wanted to act towards me in an "honourable and gentlemanly" manner, and that he would do what was "most pleasing" to me. At the same time,

with great eagerness, he proposed to me that we should jointly work out the subject to which our correspondence had referred. With this proposal, for which Professor Tyndall's letter had already prepared me, I closed with an alacrity equal to that with which it had been advanced, reserving only one point. I mentioned that, in consequence of the action taken by the British Association, I felt bound to proceed with the experiments on sunlight at Oxford, for which the apparatus had already been constructed, and for making which London was scarcely a fit place; so that the experiments to be made by Prof. Tyndall and myself at London were to be restricted to artificial radiant sources. To this condition Prof. Tyndall cheerfully agreed, observing that he had from the first intended to make experiments only on the electric light; that he was consequently quite satisfied with the arrangement proposed; and that he had anticipated that there would be no difficulty between us to come to terms. After some more conversation, and on rising to leave, I adverted to "definite arrangements" to be made for the execution of our agreement. Prof. Tyndall then stated that he was just preparing for his lectures at the School of Mines, which would take him from four to six weeks; that he found it difficult to give his mind to more than one subject at a time; and that as soon as he had set up any apparatus, he would write to me that I might come up to town from Oxford, he being sure not to make any experiments in my absence. On this understanding I left the Royal Institution, and soon after returned to Oxford. Several weeks had elapsed without my having heard from Prof. Tyndall. Having then occasion to write to him on another matter, I got an answer from him, in which occur these words:—"I have *not* been able to think of your subject." Some three weeks after that I received another short note from Prof. Tyndall, in answer to one of my own, stating that he had been too unwell for some time to write to me.

From that time further till October last year, I had no news from Prof. Tyndall. In the first days of that month, however, I returned to town for the winter, and I again called at the Royal Institution. Prof. Tyndall then told me that he had set up some apparatus in the summer (two mirrors, I believe), but that he had obtained no results. In answer to his questions, I stated to Prof. Tyndall that it was not likely that I should continue the experiments begun at Oxford; and I also referred, as far as I properly could, to the understanding that subsisted between us. Prof. Tyndall, in reply, remarked that he did not intend to take up the subject just then, but that I might rest assured that he "would do nothing" without me. A fortnight after that I had occasion to again call on Prof. Tyndall, when he

showed me the proof of the paper since published in the Philosophical Magazine for November 1864, and he invited me to read the paper in his presence, while he was perusing his letters. On reading the very first sentences of the proof, I was somewhat displeased by the marked manner in which Prof. Tyndall alluded in them to the "difference" between his reasonings and my own on the subject of lime-light. I was still more startled to learn that, whilst a fortnight ago I had been informed by Prof. Tyndall that he was occupied with different matters, he should in the meanwhile have attacked a research intimately related to that which had formed the subject of our agreement. I might have expected that, in the experiments on the spectrum of the oxy-hydrogen-flame, concerning which I was the first to enunciate the views which those experiments were intended to test, Prof. Tyndall might have invited me to join him, or, at least, to be present while they were making—the more so as I was then in town, and daily in the Library of the Royal Institution. However, my astonishment increased when I came to the passage in which Prof. Tyndall expressed his resolution not "to publish anything relating to the subject" of "the production of incandescence" by invisible Herschellic rays till the "arrangements devised by me had had a sufficient trial." Considering the agreement that had subsisted all along between us, this statement of Prof. Tyndall was rather puzzling to me. Moreover, abstracting altogether from that agreement, I saw objections in the use of the term "to publish." An unwary reader might assume that Prof. Tyndall, when writing the sentence above mentioned, had already accomplished what I was searching after, but from generosity towards myself would not publish his results. I, who knew better, and other readers, on the other hand, were obliged to assume that Prof. Tyndall intended to work out the subject privately, and to defer the publication of his results till the period referred to. A proceeding of this kind I considered an unusual one among men of science, and one fair neither to myself nor to Prof. Tyndall; as it is scarcely possible, or perhaps fit, to keep scientific results secret which are arrived at at a public place like the Royal Institution. An observer of physiognomy would have noticed that the impression which the reading of the passages adverted to had made on me was far from favourable. However, I was not prepared to make any immediate remarks, and I consequently merely stated to Prof. Tyndall that, regarding the "arrangements" referred to by him in his paper (meaning the experiments on sunlight begun at Oxford), it was unlikely that I should proceed with them for the future. I further inquired of Prof. Tyndall what day he might be at leisure, when I should be ready to "talk over" his paper with

him, of which he had given me the proof for private perusal. Prof. Tyndall replied that there was nothing to talk over; and, as I had again adverted to "definite arrangements," he stated that he had no present intention of prosecuting the matter which had been discussed between us. Upon this I left. I had then reason to expect that I should soon see Prof. Tyndall in a more private manner; and had that expectation been fulfilled, all the complication that has since arisen would have probably been avoided. However, some nine days had elapsed without my having seen Prof. Tyndall, when on Saturday the 29th of October 1864, happening to be at the Library of the Royal Institution, I was addressed by Prof. Tyndall, who had come to the Library with another person on some business of their own. On the invitation of Prof. Tyndall, I followed him to the Laboratory. On my way there Prof. Tyndall said to me, "I have been working on your subject, and I have succeeded"—or other words similar in meaning. Arrived in the Laboratory, Prof. Tyndall rendered in my presence a piece of platinized platinum-foil incandescent by the rays of an electric lamp, transmitted by a layer of bisulphide of carbon containing iodine in solution. This experiment was far from being unexceptionable, or of demonstrative power; yet it held out great hopes that the conjectures I had emitted were realizable in fact, and on seeing it my sense of pleasure was consequently great enough for a moment to obliterate all other considerations from my mind. From this state, however, I was soon after aroused, when, on leaving, Prof. Tyndall addressed me in these (or similar) words:—"We shall now make our arrangements, at least you shall see what I intend to publish in MS., and then you shall make your remarks upon it."

Let the reader imagine to himself an emigrant from these islands who, after a stormy voyage, has landed on the Western coast of North America. Guided by geological knowledge, he has sought out a remote uninhabited tract, where he has begun to dig for gold. After many months of hard labour, suffering, and privation, unrelieved by the sympathy of living being, and as yet profitless from the imperfection of his tools, the emigrant's preseverance begins to flag, when, one evening, he suddenly perceives a man in the distance, who on approaching greets him in a most affectionate manner. The new arrival is an American, who, having heard of the enterprise of the emigrant, and having great faith in the reasons which had led him to suppose that particular spot to be rich in gold, had set out to join him in his undertaking. The mere advantage of pleasant companionship would have been a sufficient inducement for the emigrant to accept of the proposal of joint work for mutual benefit which the American is pressing upon him, but the fact that

the American has in his possession tools of a superior kind renders his proposal all the more acceptable. The compact being entered into by both on the spot, and in terms more or less precise, the American alleges fatigue, and suggests that they should go to rest. On this invitation they both go to sleep, but the American soon rouses himself and begins to work with all his might while his fellow is asleep. Aided by his superior instruments, the American soon reaches with his tool into a gold-bearing vein; but at the very instant that he gets the first glimpse of the metal he catches also the open eye of his companion. "I am glad to see you are awake," then observes the American; "just get up and look, here is a pretty large 'nugget.' But don't trouble yourself any further about it. I shall soon bring it myself to the surface, and, of course, you shall have your rights. I intend to state at the mint where I mean to barter this 'nugget,' that you were the first (the emigrant had been in fact the only one) to point out this tract as a gold-bearing region, and that I have appropriated the 'nugget' and 'claim' to myself by your free consent."

As the position of the emigrant to the American, so was mine to Prof Tyndall. For, as he subsequently informed me, in the paper in which he intended to publish his results, it was his intention to state very fully my relation to it, and to show that I was the first to definitely attack it; and it was also his intention to mention that it was by my permission he published his experiments.

I, at first, proposed to await the publication of Prof. Tyndall's intended paper, and then to explain the manner of his interference in public. Wishing to avoid, however, public polemics, upon second thoughts I wrote to Prof. Tyndall a private letter, reminding him of the engagement that subsisted between us, and of all the other things that had passed between us. In reply, Prof. Tyndall asserted that I had declined in April the proposal for joint work he had made to me. With singular delicacy, Prof. Tyndall stated further that his proposal had been made only on the spur of the moment, and also that, by the experiment he had shown me, he had succeeded in "solving" the question which interested us both. I then wrote again to Prof. Tyndall, stating that, so far from my having declined his proposal in April, I had written and told to several persons that he and I were going to make joint experiments; I added, however, that, as he now stated that his proposal had been made only upon the spur of the moment, which, I supposed, meant that he had afterwards repented it, I should be sorry to insist upon any specific performance of his part of our agreement. I also observed that, however hopeful the experiment which had been shown me might be, it was as yet

far from conclusive; or else I should not have written privately on the subject, as it would have been too late to carry out our agreement. Several more letters passed between us, the result of which was that Prof. Tyndall volunteered to bind himself not to make known publicly or privately any experiments on ray-transmutation till November 1865*. When I wrote in reply that, being anxious to see the matter upon which I had been working proved, and not having any hope of being able to pursue the subject myself any further, in the interest of science I would not hold him to the engagement to which he had volunteered to bind himself, Prof. Tyndall answered that, to his regret, he could not accept the release I had offered him, his simple duty appearing to him to be strictly to adhere to the engagement to which he had voluntarily pledged himself. Notwithstanding this, Prof. Tyndall has seized the very first opportunity that offered itself to him to publish, some eleven months in advance, those very experiments which, he had asserted, he could not with any degree of satisfaction to himself publish before November 1865, as there were things more important in his estimation than the mere claims of science.

3. I now turn to reply to Prof. Tyndall's paper more in detail. In doing so, however, I shall not take any more notice of his personal animadversions than will be absolutely necessary for the vindication of my person from the insinuations levelled against me.

My "Note on Ray-Transmutation," published in the Supplementary Number of the Philosophical Magazine for December 1864, though not exclusively, was principally intended to point out a capital defect in Prof. Tyndall's published reasoning regarding the origin of lime-light, &c. Prof. Tyndall does not deny that there is a "missing link" in his argument; but, adopting a well-known forensic device, he turns round upon me, and charges me with having committed an even greater error. I had spoken, he says, of the "paucity of rays of high refrangibility in a hydrogen-flame." Now, Prof. Tyndall quotes passages from the writings of Prof. Stokes, which appear to prove the fact to be the reverse of my assumption. In doing so, however, Prof. Tyndall evidently forgets that, the richer a hydrogen-flame is in invisible Ritteric rays, the more egregious is the oversight he committed in leaving, in his own reasonings concerning the phenomena of lime-light, &c., the Ritteric rays altogether out of consideration. In the paper published in the Reports of the British Association for 1863, I certainly called the oxyhydrogen "poor" in Ritteric rays, because, according to Dr. Miller's observations, the pho-

* Prof. Tyndall added, however, that he would hold himself at liberty to repeat and develope his experiments "on combustion" by invisible rays.

tographic impression produced by an oxyhydrogen-flame in the space of 20 seconds was found to be very faint. The expressions of "poor" and "rich," however, are well known to be of comparative value only; and Prof. Tyndall is well aware that I grounded my conclusions regarding the origin of lime-light, not upon the poverty or richness of the oxyhydrogen-flame in Ritteric rays in its natural state, but upon the *comparative* abundance of Newtonic and Ritteric rays emitted by the oxyhydrogen-flame and by the lime-light. For the purpose of proving this, I need refer only to the note which Prof. Tyndall certainly had before him in writing his late paper. I there expressly state that I relied in my reasonings upon the probable poverty of the oxyhydrogen-flame in Ritteric rays, *as compared with lime-light*, which was later corroborated by the experiments of Dr. Miller*.

Not content, however, with showing up imaginary defects in my reasoning on the origin of lime-light, Prof. Tyndall attempts to prove also the existence of "radical vices" in my experimental suggestion for the production of calcescence. What I "imagine," he says, in my first proposal, "is plain enough," viz. "that the *whole heat of the flame* [the italics are Prof. Tyndall's] is radiated against one mirror and condensed by the other." And he continues, "It is not the practical difficulties, which Dr. Akin himself discerns, that I am now speaking of; it is the radical vice of the conception that a purely gaseous flame, placed in the focus of a mirror, however large, could possibly generate a temperature 'approximately equal to that of the flame itself,' in the focus of another mirror." What Prof. Tyndall means by the phrase, "the whole heat of the flame is radiated against one mirror," I am at a loss to understand; but the point on which he impugns the validity of my reasoning is stated in the latter part of the sentence quoted, and which is plain enough. Now, I would ask any one, knowing the labours of Fourier and others, what would be the consequence, *according to theory*, if at one of the foci of a vacuous ellipsoidal envelope of proper form and perfect reflecting power an oxyhydrogen-flame were placed, and at the other focus a piece of platinum, for instance? Which is the "amusing" supposition? is it to suppose that the platinum would ultimately attain to the temperature of the flame? or is it to suppose, as Prof. Tyndall does, that this latter assumption is "absurd?" "As a proposed experimental demonstration," Prof. Tyndall continues in his urbane language, "of a point which can only be decided by experiment, Dr. Akin's third proposition is, if possible, more hopelessly absurd than his first." At whose door the absurdity lies in the first case, I have already demonstrated; but this second imputed "absurdity" is

* See Phil. Mag. vol. xxviii. p. 556.

indicated in language which I am altogether at a loss to construe. Why "a proposed experimental demonstration of a point which can only be decided by experiment," should be necessarily "hopeless" and "absurd," because it is a "proposition," *i. e.* a "proposal," it is really beyond ordinary reasoning powers to comprehend. I proposed three experiments by which I hoped, with more or less certainty, to produce calcescence; to say that I advanced the mere "proposition" as equivalent to a demonstration of fact, which is the only intelligible meaning I can assign to Prof. Tyndall's words, is one of those vagaries of language peculiar to Prof. Tyndall, and to other examples of which I shall yet have to advert.

Prof. Tyndall further criticises that of my proposed experiments which he allows to be "rational"—the "only rational" one among the three which I had suggested. I had hoped to produce incandescence by means of a concentrated solar beam sifted by a diaphragm of monochromatic red glass. "It is needless to remark," Prof. Tyndall says, "that, even had this experiment succeeded, the question would have still remained unsolved; for a sheet of glass, which permits the most powerful rays of the visible spectrum to pass through it, could not be called a 'proper absorbent.'" I assert that red glass *is* a "proper absorbent." I had proposed to myself a double object:—first, to produce incandescence by invisible Herschellic rays; and secondly, to produce visible or Newtonic rays of a given refrangibility in incandescent substances, by the incidence of other visible rays of inferior refrangibility—for instance, green rays by the incidence of red rays*. This latter experiment, although less striking to the eye than the transmutation of invisible Herschellic rays into visible or Newtonic rays, would yet possess quite as great an importance as the first experiment in a theoretical point of view; and for its realization red glass is a "proper absorbent." I was, however, at no loss regarding absorbents capable of separating the invisible Herschellic rays *wholly* from the visible or Newtonic rays. From Melloni's memoirs, I knew that black glass of a certain variety is an absorbent of this kind; and from Prof. Tyndall's work on 'Heat'—many months before he "performed the thankless task of communicating to me *his* ideas" by word of mouth—I had learned that a solution of iodine in bisulphide of carbon possessed similar qualities. Ever since 1863 I endeavoured to procure black glass, in which, till lately, I did not succeed; and in the spring of 1863 (that is, many months before Prof. Tyndall's thoughts had turned, upon his own showing, to the subject of ray-transmutation as exemplified in the lime-light,

* Prof. Tyndall only shows how little he understands the subject even now, when he says that the "real problem" may be broadly stated thus:—"*To raise the refrangibility of invisible rays of long period, so as to convert them into visible rays.*"

&c.) I had caused to be constructed a peculiar diaphragm for the express purpose of containing iodine in solution, and I had made those observations on the spectrum of that solution which I have adverted to in the Philosophical Magazine for December 1864. Prof. Tyndall asks whether it will be believed that he needed my "ideas" to inform him what he was "to do with the obscure radiation from the electric light?" Why should it not be believed? Every one knew that the invisible part of the radiation of the sun is calorifically powerful—at least to the same extent as Prof. Tyndall had shown that of the electric light to be by the experiments to which he refers in his last paper. Every one who had read Melloni's papers knew also that, by means of black glass, the Herschellic rays may be separated from the Newtonic; and yet nobody knew what he was "to do" with the obscure radiation of the sun. In the article published in the 'Saturday Review' the solution proposed by me for the problem which I had indicated was compared with the solution proposed in the case of "Columbus's egg;" and as in that case, so also in mine, it is easy to say, *ex post facto*, that anyone might have found the solution, had he but bethought himself of it.

4. As regards Prof. Tyndall's attempted criticism of my published papers on the subject of calcescence, to which I have now replied, I might have perhaps saved myself the trouble of answering it. One of the Secretaries of the Royal Society wrote to me, after reading those papers, that they showed "a perfect mastery" of the subjects treated of; and relying upon this judgment, I could have afforded to contemn Prof. Tyndall's adverse opinions. I now have to reply to those statements of Prof. Tyndall which concern more my person than my writings.

Prof. Tyndall taunts me with "sitting down and proposing experiments which may, or may not, be capable of realization. At all events if this be done at all, it ought to be done in a magnanimous spirit," and not "with a view to mounting the high horse of Neptune." This phrase, again, appears to me somewhat difficult to understand; but whatever its meaning, Prof. Tyndall totally misrepresents what I have done. For more than a year I was in possession of and had matured my ideas ere I put pen to paper with a view to publication; and when, in August 1863, I read my papers at the British Association, it was not with a view of mounting any "high horse of Neptune," but with a view to induce that body to help me to execute the experiments I had proposed*.

* The readers of the Philosophical Magazine who remember Prof. Tyndall's recent diatribes against Mr. Joule, will but smile at the consistency he exhibits in attempting to cry down at present the value of theoretical speculations unsupported by experiment.

Prof. Tyndall regrets that he was not more "suspicious" and I more "open"; and he employs in this connexion several other expressions in keeping with his usual urbanity. No doubt Prof. Tyndall's and my own views on this latter subject differ considerably. I have stated already that in October 1864 he gave me a duplicate proof of the paper he was then publishing. Having read it in his presence, I proposed to "talk it over" with him, when he replied that "there was nothing to talk over." Had I shared Prof. Tyndall's views regarding conduct, no doubt I should have been more "outspoken," and should have pressed upon him the remarks I wished to make, notwithstanding. As to the "rule of courtesy in this country," as in others, regarding the publication of private correspondence, I am very well aware of it, and have acted accordingly. I have not published any of Prof. Tyndall's letters to me, nor have I committed that worse offence than a breach of courtesy, which he seems to be willing to lay at my door. He "challenges" me "expressly" to publish the letter which I received from him from the Isle of Wight, and from which I have quoted two words. I hold that letter at Prof. Tyndall's disposal, for him to publish it entirely if he chooses to do so. For the vindication of my own good faith, it will be sufficient if I transcribe the whole of the sentence from which I have made the quotation to which Prof. Tyndall objects. It is as follows:—"As to the possibility of converting the Herschellic rays into Newtonic rays, I do not entertain a doubt; and indeed the piece of work which I had set before me for attack on my return from this place was a series of experiments on this very subject." This letter was written in April 1864; the article in the 'Saturday Review,' to which it in another part refers, appeared in January 1864. This is substantially what I have stated in the Philosophical Magazine for December last.

Prof. Tyndall is mistaken in stating that I had written to him a "friendly note" twenty-four hours before my last article was published in this Magazine. I simply wrote to him a formal note, accompanying a book-parcel, which in itself was the indication of a rupture. That both were not sent a week or two earlier was the fault of the bookbinder.

It is evidently the same undue reliance on his superiority of age and seniority as a physicist, that, as he fancied, entitled him to adopt a tone of affected disregard towards me, which emboldened also Prof. Tyndall to state that, had I remained silent for some indefinite period, and shown myself sufficiently meek, "he had intended to give me an opportunity of attaching my name to the experiments he had been making." But in this Prof. Tyndall entirely misrepresents our relative positions. It

was I who, at his request, had granted him a share in the cultivation of a field which I had opened up*, and upon which, as he has himself acknowledged to me, he has since been but trespassing; and it was not a little preposterous to expect, to say the least of it, that I would consent to accept as a boon, or stoop to petition for, a share in the produce of what was my own undoubted intellectual property.

I have understood that the well-known fate of Dr. Mayer, in whose behalf Prof. Tyndall has written so feelingly and magnanimously, was brought about by the vexation he encountered in connexion with his speculations on Heat. In former years I should have considered such a statement as a mere piece of sentimental invention. The experience of the last three years, however, has taught me different; only, while with certain constitutions continued vexation of this kind, coupled as it but too often is with troubles of another kind, attacks the brain, with different constitutions it undermines other, but not less vital, parts of the system. Prof. Tyndall, who is charitable enough to express an implied wish for my non-existence, will no doubt be gratified to learn that the experience I have met with at his hands has acted upon me in the manner he would seem to desire. Yet, strange as it may appear, there are moments when I myself am inclined to palliate his conduct. The colleague of a Faraday, and the successor, virtual or real, of a Davy and a Young, may well consider himself under the obligation to enrich science by at least one signal discovery. Circulars issued by the Royal Institution might ascribe to him the discovery of the Absorption of Heat by Gases; too kind and partial patrons or friends might speak of the "lustre" of his researches; but Prof. Tyndall well knew that the discovery of the absorption of heat by gases—or even that of the so-called "principles" of "dynamic radiation," and of "accord and discord"—was not his, and that the lustre of his researches, however meritorious some of them might be, had been hitherto far from dazzling. But here there was a genuine discovery, which the best authority on the subject had said would be a great one, and which, though virtually made, yet, being still in want of ocular demonstration, almost any one possessed of the necessary instruments might attach his name to. What neither the qualities of his mind nor the genius of the scene of his labours had hitherto supplied him with, seemed thus suddenly placed within Prof. Tyndall's reach. By writing to the editor of the 'Saturday Review'†, Prof. Tyndall attempted to claim the discovery in question for

* At our interview in April, at which Prof. Tyndall seems to have had a clearer conception of our mutual position than before or ever after, he observed to me that he would not "touch at the subject" on which I was making experiments unless I "permitted" him.

† Prof. Tyndall, having read the article previously referred to in the

himself. Later, when I myself brought about that immediate communication which he seems to have desired the editor of the 'Saturday Review' to establish between us, he claimed, or rather requested, a share in it. But no sooner was that share (perhaps too readily) granted, than his appetite grew again, and he wanted once more the whole. When I checked him in this desire, in November Prof. Tyndall had a momentary qualm of conscience; but this appears to have passed off since; and in the last Number of the Philosophical Magazine Prof. Tyndall's sense of right appears again as obdurate as ever. It would be a blunder in psychology to suppose that proceedings of this kind arise from conscious motives, or that they are carried on with a full knowledge of their injustice. Greed is too powerful an instinct to allow of much reflection, and Ambition too ingenious a sophist not to be able to represent black as white, foul as fair, if need be. However that may be, Prof. Tyndall has assured me that, when I should have achieved and published "the great experiment" I was striving to effect, he would muster sufficient greatness of heart not to envy me. I can say, now that Prof. Tyndall has tried to anticipate me, that, whatever advantage he may derive from that fact, I shall not envy him the means by which he has gained it. For my own part, I shall endeavour to console myself with thinking that it is the destiny of some to sow for others to reap.

Paris, January 1865.

Corrigenda in the January Number.

Page 40, line 7 from below, *for* always *read* generally.
— 42, — 13, *for* are greater than those *read* is greater than that.

[It is with considerable regret we find ourselves called upon to

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'Saturday Review,' actually wrote to the editor to say (as he himself is at the pains to state) that he had "accomplished already" what I "proposed to accomplish." This statement of Prof. Tyndall either shows how little he understood, even so late as January last year, the conditions of the problem to be solved; or else it forms an abuse of language rarely exemplified in the annals of science. I intended to produce calcescence by incident *rays*; Prof. Tyndall, on the other hand, had guessed (or perhaps only remembered what I had emitted before him) that the common phenomena of incandescence exhibited by solids in *contact* with flames were owing to an effect of a similar, but far from identical, nature to that which I intended to realize; and relying upon this fact, he considered himself justified in saying that *he* had "already accomplished" what I intended to accomplish. Really I am wanting in words strong enough to characterize a statement of this nature. It is as if somebody, having heard that Newton intended to verify the idea of universal gravitation by applying it to the motion of the moon, had asserted himself to have "accomplished" that already, because it had occurred to him that the fall of an apple might be due to the attraction of the earth.

Prof. Tyndall invited the editor of the 'Saturday Review' to communicate to me his letter, with what view he does not state. Prof. Tyndall might have learnt from the editor of the 'Saturday Review,' whom he cites as a witness, that he never communicated with me on the subject.

devote so much space to these discussions of purely personal matters; and although we have, in justice to our subscribers and correspondents, given in this as well as in former instances an additional half sheet, we must now state that after admitting, as in fairness bound to do, the publication of any reply that Prof. Tyndall may possibly forward for insertion, the discussion of this subject must close, so far as our pages are concerned.—W. F.]

XXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 75.]

November 17, 1864.—Major-General Sabine, President, in the Chair.

THE following communication was read:—

"On the Spectra of some of the Nebulæ." By W. Huggins, Esq., F.R.A.S.;—a Supplement to the Paper "On the Spectra of some of the Fixed Stars," by W. Huggins, Esq., and W. A. Miller, M.D., Treas. and V.P.R.S.

The author commences by showing the importance of bringing analysis by the prism to bear upon the remarkable class of bodies known as nebulæ, especially since the results obtained by the largest telescopes hitherto constructed appear to show that increase of optical power alone would probably fail to determine the question whether all the nebulæ are clusters of stars too remote to be separately visible.

The little indication of resolvability, the absence of central condensation, the greenish-blue colour, and the intrinsic brightness characterizing many of the nebulæ classed by Sir W. Herschel as planetary, induced the author to select chiefly nebulæ of this class for prismatic observation.

The apparatus employed is that of which a description is given in the paper, "On the Spectra of some of the Fixed Stars," by the author and Dr. W. A. Miller, to which this is a supplement.

No. 4373*, 37 H. IV. Draconis. A bright planetary nebula, with a very small nucleus. The light from this nebula is not composed of light of different refrangibilities, and does not therefore form a continuous spectrum. It consists of light of three definite refrangibilities only, and, after passing through the prisms, remains concentrated in three bright lines.

The strongest of these occupies a position in the spectrum about midway between *b* and *F*, and was found, by the method of simultaneous observation, to be coincident with the brightest of the lines of nitrogen.

A little more refrangible, a second line is seen. At about three times the distance of the second line, a third, very faint line occurs; this coincides in position with Fraunhofer's *F*, and one of the lines of hydrogen. Besides the three bright lines, an exceedingly faint continuous spectrum of the central bright point was perceived.

* These numbers refer to the last catalogue of Sir J. F. W. Herschel, Phil. Trans. Part I. 1864, pp. 1–138.

The planetary nebula, 4390, Σ 6, Tauri Poniatawskii; 4514, 73 II. IV. Cygni; 4510, 51 II. IV. Sagittarii; 4628, 1 II. IV. Aquarii; 4964, 18 II. IV., the annular nebula in Lyra 4447, 57 M., and the Dumb-bell in Vulpecula 4532, 27 M., gave spectra identical with the spectrum of 37 H. IV., except that in the case of some of these the strongest only of the three bright lines was seen.

It is obvious that these nebulæ can no longer be regarded as clusters of stars. In place of an incandescent solid or liquid body transmitting light of all refrangibilities through an atmosphere which intercepts by absorption some of them, such as our sun and the fixed stars appear to be, these nebulæ, or at least their photosurfaces, must be regarded as enormous masses of luminous gas or vapour.

On this supposition the absence of central condensation admits of explanation; for even if the whole mass of the gas is luminous, the light emitted by the portion of gas beyond the surface visible to us would be in great measure absorbed by the portion of gas through which it would have to pass, and for this reason there would be presented a luminous surface only. The small brilliancy of the nebulæ, notwithstanding the considerable angle which in most cases they subtend, is in accordance with the very inferior splendour of glowing gas as compared with incandescent solid or liquid matter.

The extreme simplicity of constitution which the three bright lines suggest, whether or not we regard them as indicating the presence of nitrogen, hydrogen, and a substance unknown, is opposed to the opinion that they are clusters of stars.

The following nebulæ and resolvable clusters gave a continuous spectrum:—4294, 92 M. Herculis; 4244, 50 H. IV. Herculis; 116, 31 M., the Great Nebula in Andromeda; 117, 32 M. Andromedæ; 428, 55, Andromedæ; 826, 26 H. IV. Eridani.

In the spectrum of 31 M., the nebulæ in Andromeda, and in that of the companion nebula, 32 M., the red and part of the orange are wanting.

December 8.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

"A Dynamical Theory of the Electromagnetic Field." By Professor J. Clerk Maxwell, F.R.S.

The proposed Theory seeks for the origin of electromagnetic effects in the medium surrounding the electric or magnetic bodies, and assumes that they act on each other not immediately at a distance, but through the intervention of this medium.

The existence of the medium is assumed as probable, since the investigations of Optics have led philosophers to believe that in such a medium the propagation of light takes place.

The properties attributed to the medium in order to explain the propagation of light are—

1st. That the motion of one part communicates motion to the parts in its neighbourhood.

2nd. That this communication is not instantaneous but progressive, and depends on the elasticity of the medium as compared with its density.

The kind of motion attributed to the medium when transmitting light is that called transverse vibration.

An elastic medium capable of such motions must be also capable of a vast variety of other motions, and its elasticity may be called into play in other ways, some of which may be discoverable by their effects.

One phenomenon which seems to indicate the existence of other motions than those of light in the medium, is that discovered by Faraday, in which the plane of polarization of a ray of light is caused to rotate by the action of magnetic force. Professor W. Thomson* has shown that this phenomenon cannot be explained without admitting that there is motion of the luminiferous medium in the neighbourhood of magnets and currents.

The phenomena of electromotive force seem also to indicate the elasticity or tenacity of the medium. When the state of the field is being altered by the introduction or motion of currents or magnets, every part of the field experiences a force, which, if the medium in that part of the field is a conductor, produces a current. If the medium is an electrolyte, and the electromotive force is strong enough, the components of the electrolyte are separated in spite of their chemical affinity, and carried in opposite directions. If the medium is a dielectric, all its parts are put into a state of electric polarization,† a state in which the opposite sides of every such part are oppositely electrified, and this to an extent proportioned to the intensity of the electromotive force which causes the polarization. If the intensity of this polarization is increased beyond a certain limit, the electric tenacity of the medium gives way, and there is a spark or "disruptive discharge."

Thus the action of electromotive force on a dielectric produces an electric displacement within it, and in this way stores up energy which will reappear when the dielectric is relieved from this state of constraint.

A dynamical theory of the Electromagnetic Field must therefore assume that, wherever magnetic effects occur, there is matter in motion, and that, wherever electromotive force is exerted, there is a medium in a state of constraint; so that the medium must be regarded as the recipient of two kinds of energy—the actual energy of the magnetic motion, and the potential energy of the electric displacement. According to this theory we look for the explanation of electric and magnetic phenomena to the mutual actions between the medium and the electrified or magnetic bodies, and not to any direct action between those bodies themselves.

In the case of an electric current flowing in a circuit A, we know that the magnetic action at every point of the field depends on its position relative to A, and is proportional to the strength of the current. If there is another circuit B in the field, the magnetic effects due to B are simply added to those due to A, according to the well-known law of composition of forces, velocities, &c. According to our theory, the motion of every part of the medium depends partly on the strength of the current in A, and partly on that in B, and

* Proceedings of the Royal Society June 1856 and June 1861.

when these are given the whole is determined. The mechanical conditions therefore are those of a system of bodies connected with two driving-points A and B, in which we may determine the relation between the motions of A and B, and the forces acting on them, by purely dynamical principles. It is shown that in this case we may find two quantities, namely, the "reduced momentum" of the system referred to A and to B, each of which is a linear function of the velocities of A and B. The effect of the force on A is to increase the momentum of the system referred to A, and the effect of the force on B is to increase the momentum referred to B. The simplest mechanical example is that of a rod acted on by two forces perpendicular to its direction at A and at B. Then any change of velocity of A will produce a force at B, unless A and B are mutually centres of suspension and oscillation.

Assuming that the motion of every part of the electromagnetic field is determined by the values of the currents in A and B, it is shown—

1st. That any variation in the strength of A will produce an electromotive force in B.

2nd. That any alteration in the relative position of A and B will produce an electromotive force in B.

3rd. That if currents are maintained in A and B, there will be a mechanical force tending to alter their position relative to each other.

4th. That these electromotive and mechanical forces depend on the value of a single function M, which may be deduced from the form and relative position of A and B, and is of one dimension in space; that is to say, it is a certain number of feet or metres.

The existence of electromotive forces between the circuits A and B was first deduced from the fact of electromagnetic attraction, by Professor Helmholtz* and Professor W. Thomson†, by the principle of the Conservation of Energy. Here the electromagnetic attractions, as well as the forces of induction, are deduced from the fact that every current when established in a circuit has a certain persistency or momentum—that is, it requires the continued action of an unresisted electromotive force in order to alter its value, and that this "momentum" depends, as in various mechanical problems, on the value of other currents as well as itself. This momentum is what Faraday has called the Electrotonic State of the circuit.

It may be shown from these results, that at every point in the field there is a certain direction possessing the following properties :—

A conductor moved in that direction experiences no electromotive force.

A conductor carrying a current experiences a force in a direction perpendicular to this line and to itself.

A circuit of small area carrying a current tends to place itself with its plane perpendicular to this direction.

A system of lines drawn so as everywhere to coincide with the direction having these properties is a system of lines of magnetic

* Conservation of Force. Berlin, 1847: translated in Taylor's Scientific Memoirs, Feb. 1853, p. 114.

† Reports of British Association, 1848. Phil. Mag. Dec. 1851.

force; and if the lines in any one part of their course are so distributed that the number of lines enclosed by any closed curve is proportional to the "electric momentum" of the field referred to that curve, then the electromagnetic phenomena may be thus stated:—

The electric momentum of any closed curve whatever is measured by the number of lines of force which pass through it.

If this number is altered, either by motion of the curve, or motion of the inducing current, or variation in its strength, an electromotive force acts round the curve and is measured by the decrease of the number of lines passing through it in unit of time.

If the curve itself carries a current, then mechanical forces act on it tending to increase the number of lines passing through it, and the work done by these forces is measured by the increase of the number of lines multiplied by the strength of the current.

A method is then given by which the coefficient of self-induction of any circuit can be determined by means of Wheatstone's electric balance.

The next part of the paper is devoted to the mathematical expression of the electromagnetic quantities referred to each point in the field, and to the establishment of the general equations of the electromagnetic field, which express the relations among these quantities.

The quantities which enter into these equations are:—Electric currents by conduction, electric displacements, and Total Currents; Magnetic forces, Electromotive forces, and Electromagnetic Momenta. Each of these quantities being a directed quantity, has three components; and besides these we have two others, the Free Electricity and the Electric Potential, making twenty quantities in all.

There are twenty equations between these quantities, namely Equations of Total Currents, of Magnetic Force, of Electric Currents, of Electromotive Force, of Electric Elasticity, and of Electric Resistance, making six sets of three equations, together with one equation of Free Electricity, and another of Electric Continuity.

These equations are founded on the facts of the induction of currents as investigated by Faraday, Felici, &c., on the action of currents on a magnet as discovered by Oersted, and on the polarization of dielectrics by electromotive force as discovered by Faraday and mathematically developed by Mossotti.

An expression is then found for the intrinsic energy of any part of the field, depending partly on its magnetic, and partly on its electric polarization.

From this the laws of the forces acting between magnetic poles and between electrified bodies are deduced, and it is shown that the state of constraint due to the polarization of the field is such as to act on the bodies according to the well-known experimental laws.

It is also shown in a note that, if we look for the explanation of the force of gravitation in the action of a surrounding medium, the constitution of the medium must be such that, when far from the presence of gross matter, it has immense intrinsic energy, part of which is removed from it wherever we find the signs of gravitating force. This result does not encourage us to look in this direction for the explanation of the force of gravity.

The relation which subsists between the electromagnetic and the electrostatic system of units is then investigated, and shown to depend upon what we have called the Electric Elasticity of the medium in which the experiments are made (*i. e.* common air). Other media, as glass, shellac, and sulphur have different powers as dielectrics; and some of them exhibit the phenomena of electric absorption and residual discharge.

It is then shown how a compound condenser of different materials may be constructed which shall exhibit these phenomena, and it is proved that the result will be the same though the different substances were so intimately intermingled that the want of uniformity could not be detected.

The general equations are then applied to the foundation of the Electromagnetic Theory of Light.

Faraday, in his "Thoughts on Ray Vibrations"*, has described the effect of the sudden movement of a magnetic or electric body, and the propagation of the disturbance through the field, and has stated his opinion that such a disturbance must be entirely transverse to the direction of propagation. In 1846 there were no data to calculate the mathematical laws of such propagation, or to determine the velocity.

The equations of this paper, however, show that transverse disturbances, and transverse disturbances only, will be propagated through the field, and that the number which expresses the velocity of propagation must be the same as that which expresses the number of electrostatic units of electricity in one electromagnetic unit, the standards of space and time being the same.

The first of these results agrees, as is well known, with the undulatory theory of light as deduced from optical experiments. The second may be judged of by a comparison of the electromagnetical experiments of Weber and Kohlrausch with the velocity of light as determined by astronomers in the heavenly spaces, and by M. Foucault in the air of his laboratory.

Electrostatic units in an electromagnetic unit .... } 310,740,000 metres per second.

Velocity of light as found by M. Fizeau 314,858,000.

Velocity of light by M. Foucault..... 298,000,000.

Velocity of light deduced from aberration ..... } 308,000,000.

At the outset of the paper, the dynamical theory of the electromagnetic field borrowed from the undulatory theory of light the use of its luminiferous medium. It now restores the medium, after having tested its powers of transmitting undulations, and the character of those undulations, and certifies that the vibrations are transverse, and that the velocity is that of light. With regard to normal vibrations, the electromagnetic theory does not allow of their transmission.

What, then, is light according to the electromagnetic theory? It consists of alternate and opposite rapidly recurring transverse magnetic disturbances, accompanied with electric displacements, the direc-

* Phil. Mag. 1846. Experimental Researches, vol. iii. p. 447.

tion of the electric displacement being at right angles to the magnetic disturbance, and both at right angles to the direction of the ray.

The theory does not attempt to give a mechanical explanation of the nature of magnetic disturbance or of electric displacement, it only asserts the identity of these phenomena, as observed at our leisure in magnetic and electric experiments, with what occurs in the rapid vibrations of light, in a portion of time inconceivably minute.

This paper is already too long to follow out the application of the electromagnetic theory to the different phenomena already explained by the undulatory theory. It discloses a relation between the inductive capacity of a dielectric and its index of refraction. The theory of double refraction in crystals is expressed very simply in terms of the electromagnetic theory. The non-existence of normal vibrations and the ordinary refraction of rays polarized in a principal plane are shown to be capable of explanation; but the verification of the theory is difficult at present, for want of accurate data concerning the dielectric capacity of crystals in different directions.

The propagation of vibrations in a conducting medium is then considered, and it is shown that the light is absorbed at a rate depending on the conducting-power of the medium. This result is so far confirmed by the opacity of all good conductors, but the transparency of electrolytes shows that in certain cases vibrations of short period and amplitude are not absorbed as those of long period would be.

The transparency of thin leaves of gold, silver, and platinum cannot be explained without some such hypothesis.

The actual value of the maximum electromotive force which is called into play during the vibrations of strong sunlight is calculated from Pouillet's data, and found to be about 60,000,000, or about 600 Daniell's cells per metre.

The maximum magnetic force during such vibrations is ·193, or about $\frac{1}{10}$ of the horizontal magnetic force at London.

Methods are then given for applying the general equations to the calculation of the coefficient of mutual induction of two circuits, and in particular of two circles the distance of whose circumferences is small compared with the radius of either.

The coefficient of self-reduction of a coil of rectangular section is found and applied to the case of the coil used by the Committee of the British Association on Electrical Standards. The results of calculation are compared with the value deduced from a comparison of experiments in which this coefficient enters as a correction, and also with the results of direct experiments with the electric balance.

GEOLOGICAL SOCIETY.

[Continued from p. 77.]

December 7, 1864.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read :—

1. "On the Geology of Otago, New Zealand." By James Hector, M.D., F.G.S. In a letter to Sir R. I. Murchison, K.C.B., F.R.S., F.G.S.

The south-western part of the province of Otago is composed of

crystalline rocks forming lofty and rugged mountains, and intersected by deeply cut valleys which are occupied by arms of the sea on the west, and by the great lakes on the east. These crystalline rocks comprise an ancient contorted gneiss, and a newer (probably not very old) series of hornblende-slate, gneiss, quartzite, &c. Eastwards they are succeeded by well bedded sandstones, shales, and porphyritic conglomerates, with greenstone-slates, &c., in patches, all probably of Lower Mesozoic age. Then follow the great auriferous schistose formations, which comprise an Upper, a Middle, and a Lower portion; and upon these occur a series of Tertiary deposits, the lowest of which may, however, possibly be of Upper Mesozoic date, while the upper, consisting of a Freshwater and a Marine series, are unconformable to it, and are decidedly much more recent.

In describing the auriferous formations, Dr. Hector stated that the quartz-veins occurring in the schists were not often true "fissure-reefs" (that is, reefs that cut the strata nearly vertically and have a true back, or wall, independent of the foliation-planes) but are merely concretionary laminæ that conform to the planes of foliation; the gold occurs segregated in the interspaces of this contorted schist, but is rarely found *in situ*.

Dr. Hector concluded with some remarks on the early Tertiary volcanic rocks, observing that the period of their eruption must have been one of upheaval, and that the great depth of the valleys, which have been excavated by glacier-action since the close of that period, proves that the elevation of the island, at least in the mountain-region, must once have been enormously greater than it now is.

2. "Note on communicating the Notes and Map of Dr. Julius Haast, upon the Glaciers and Rock-basins of New Zealand." By Sir R. I. Murchison, K.C.B., F.R.S., F.G.S.

In this note Sir Roderick Murchison states that Dr. Haast has informed him in a letter that he has for the last five years attentively followed the discussions on Glacier theories, that in March 1862 he came, independently of other authors, to the same conclusions in New Zealand that Professor Ramsay did in Europe, and that his views have been printed in his Colonial Reports as Geologist of the Province of Canterbury.

Sir Roderick also stated that the constant field and other occupations of Dr. Haast have hitherto prevented his carrying out his intention of writing a paper for the Geological Society; but he has sent the following notes as a *résumé* of his views.

Though opposed to the theory of the excavation of basins in hard rocks by the action of ice, Sir Roderick commended the researches of Dr. Haast as showing the mutations of the surface in successive geological periods.

3. "Notes on the Causes which have led to the Excavation of deep Lake-basins in hard Rocks in the Southern Alps of New Zealand." By Julius Haast, Ph.D., F.G.S.

Referring first to the submergence of New Zealand during the

Pliocene period, and to its subsequent elevation, the author stated that the chief physical feature of the country after that elevation was a high mountain-range, from which glaciers of enormous volume, owing to peculiar meteorological conditions, descended into the plain below, removing in their course the loose Tertiary strata, and thus widening and enlarging the pre-existing depressions, the occurrence of which had at first determined the course of the glaciers.

The author then observes that, the country having acquired a temporary stability, the glaciers became comparatively stationary, and therefore formed moraines, the materials of which were cemented together by the mud deposited from the water issuing from the glacier; new moraine matter would then raise the bed of the outlet and dam up the water below the glacier, and from this moment, he believes, the formation and scooping out of the rock-basin begins; for the ice being pressed downwards, and prevented by the moraine from descending, its force would be expended in excavating a basin in the rock below.

4. "Note on a Sketch Map of the Province of Canterbury, New Zealand, showing the glaciation during the Pleistocene and Recent times, as far as explored." By Julius Haast, Ph.D., F.G.S.

This paper contained a general explanation of a Sketch Map illustrating the past and present distribution of the glaciers on the eastern side of the Southern Alps of New Zealand, as well as the author's views on the excavation of Lake-basins in hard rocks, as shown by the coincidence between the positions of the lakes and the terminations of the ancient glaciers.

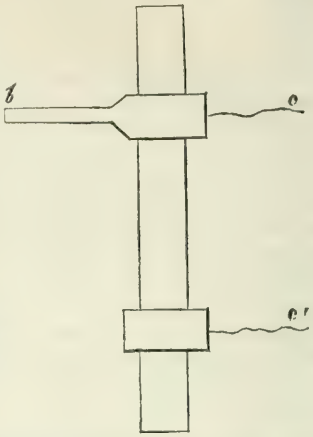
XXV. *Intelligence and Miscellaneous Articles.*

ON SOME THERMO-ELECTRIC PILES OF GREAT ACTIVITY.
BY PROF. R. BUNSEN.

OF all substances whose thermo-electric differences have been hitherto examined, and whose electric conductibility is sufficiently good to enable them to be employed advantageously in thermo-electric piles, bismuth occupies the highest, and an alloy of two parts of antimony with one part of tin the lowest place in the tension series. Experiment has shown me that pyrolusite stands above bismuth in this series, and that copper pyrites occupies a far higher place than even pyrolusite. When copper pyrites is combined with the above alloy so as to form a thermo-electric pair, or, better still—in order to be able to employ greater differences of temperature—when copper pyrites is combined with copper, far stronger currents are obtained han, under the same circumstances, are yielded by any of the er mo-electric piles hitherto in general use.

To determine the constants of such a pile the following arrange-

ment was employed. The adjoining figure represents a plate of copper pyrites 40 millims. broad, 70 long, and 7 thick; into it are inserted, at a distance of 35 millims. from each other, two copper pins of somewhat conical form, and having a mean diameter of 9 millims. Both pins are very carefully cut and platinum-plated, and the upper one has a copper projection *b*. This projection being heated by a non-luminous lamp-flame, whilst the lower part of the plate of pyrites, together with the copper pin fixed therein, is cooled in water, a current is obtained in the closed circuit formed by the copper wires *c c*, whose intensity after some time becomes constant—care being taken to protect from fluctuations the flame applied to the projection. The constants of this small pile were compared with those of a Daniell's element, arranged in the customary way. The copper surface which, in this element, was immersed in the liquid opposite to the cylinder of amalgamated zinc had an area of one square decimetre; the liquids consisted of a completely saturated solution of sulphate of copper, and of a mixture of six parts (by weight) of water and one of sulphuric acid.



If L denote the effective resistance of a pile, E its electromotive force, ω the resistance in the closed circuit, and I the intensity of the current, we have, as is well known, the relation

$$I = \frac{E}{L+\omega}.$$

The resistance being increased by r, the diminished intensity of the current will be expressed by

$$i = \frac{E}{L+\omega+r}.$$

To determine E and L, the current was passed through a coil of wire placed, in the requisite manner, at the distance d from a magnetometer, and the deflections I and i were measured which corresponded to the resistances $(L+\omega)$ and $(L+\omega+r)$. In all experiments ω and r had the values 4·46 and 16·00, respectively. The distance d of the coil of wire from the magnetometer was 1 metre with the Daniell's element, 0·5 of a metre with the pile of copper pyrites, and 0·25 of a metre with the pile of pyrolusite. Accordingly, when comparing the piles, we shall have to employ the formulæ

$$L = \frac{16i}{I-i} - 4{\cdot}46,$$

$$E = \left(\frac{16i}{I-i} + 16\right) id^3.$$

For the Daniell's element the following values were found:—

	Newly filled.	After 15 minutes' use.	Mean.
I	154·0	141·7	
i	71·0	69·2	
L	9·2	10·8	10·0
E	2108·0	2164·0	2136·0

In the experiments with the above-described plate of copper pyrites four different but constant applications of heat were tried. The following series of results were obtained (arranged according to ascending degrees of applied heat):—

	First.	Second.	Third.	Fourth.
I	98·9	116·0	134·5	150·5
i	41·8	50·7	56·7	63·5
L	7·4	7·9	7·2	7·2
E	145·4	180·2	196·0	218·8
$\frac{\text{E (ther. elec.)}}{\text{E (Daniell)}}$	$\frac{1}{14·7}$	$\frac{1}{11·9}$	$\frac{1}{10·9}$	$\frac{1}{9·7}$
$\frac{\text{L (ther. elec.)}}{\text{L (Daniell)}}$	0·74	0·79	0·72	0·72

During the experiments the heat of the water used for cooling rose somewhat over 60° C., and then remained constant at this temperature. Although the copper pyrites was heated above the temperature of melting tin, no change was observed either in its interior or on the cut surfaces of the holes pierced through it. In order not to split the apparatus, I did not apply a higher temperature; for it was manifest, from the loosening of the copper pins during the process of cooling, that, when heated, the latter metal expanded more than the pyrites. A still greater electromotive force, however, might readily be obtained by raising the temperature, provided the consequences of unequal expansion were guarded against by cutting the pin, longitudinally, with a saw down to its axis, and thus enabling it, in virtue of its elasticity, to accommodate itself to the aperture. But even at the temperature to which it was exposed, this small pile exhibited a ten times greater action than a bismuth and antimony element, of equal effective resistance, when heated from 0° to 100° C. Ten of the above-described pairs formed into a battery suffice to give all the actions of a Daniell's element, containing an effective copper surface 14 square centimetres in area.

Copper pyrites in its natural state melts easily, at a strongly glowing heat, without sensible decomposition; and it may then be cast in any mould whatever. It is a remarkable fact, however, that this substance thereby suffers a change, in consequence of which it sinks far below bismuth in the thermo-electric tension series. Consequently it is only the mineral in its natural state which can be

employed for such thermo-electric piles; in that state, however, it can be easily worked into any required form.

Pyrolusite, combined with platinum, also gives a pile whose electromotive force can be easily raised to one-tenth of that of a Daniell's element, without fear of the heat thereby required decomposing the mineral. The upper and lower ends of a small cylinder, 6 millims. in diameter and 50 millims. long, easily cut from a complicated fibrous variety of pyrolusite, were enveloped with platinum wire; and the upper junction, in a cover of mica, was heated directly in a non-luminous flame, whilst the lower one was immersed in water. An experiment therewith, in which, as already remarked, d was equal to 0·25 of a metre, gave $I=74·0$ and $i=68·2$; whence result the values $L = 183·6$ and $E = 217·5$. The electromotive force, therefore, amounted again to not less than $\frac{1}{9·8}$ of that of a Daniell's element; the resistance, however, was 18·4 times that in the Daniell's element above described.—Poggendorff's *Annalen*, vol. cxxiii. p. 505.

ON THE RADIANT HEAT OF THE MOON.

To Professor Tyndall, F.R.S., &c.

Sir, Utrecht, January 6, 1865.

Allow me to trouble you for your duly and highly esteemed opinion on a subject to which I have given considerable time and care, as you may know from my *Changements périodiques de température dépendants du Soleil et de la Lune*, and from the *Annalen* of Poggendorff. I remember your having written in the Philosophical Magazine*, first, that you could not, as Melloni did, find a heating effect from the full moon's rays by means of your thermoscope, but that, at the same time, you were of opinion that the smoke of London was an obstacle; and secondly, that you supposed that the moon's rays cleared the sky, and that from that cause the earth lost more heat than it gained by the direct action of the moon. I want to know whether this was a *lapsus calami*, or your real opinion.

It is very improbable to me that a source of heat should be in any way a source of cold, even if I could admit Sir John Herschel's opinion to be a true one—that the moon *does* clear the sky. I could not find, from many years' observations, that the sky is more serene when the moon is above than when she is beneath the horizon—in other words, that it is clearer about the time of full moon than it is about the time of new moon.

As for the influence on the temperature, I have added together all the observations taken on the first day of the moon, from 1729 till now, as well as all the observations taken on the second, third, fourth, and so on.

The result is not the same from 1729 to 1789 as it is from 1789 to now. In the first period I found a greater sum of temperatures for the days between the 12th and 26th than for the 27th, 28th, 29th, 30th, and the eleven first days. In the second period I find

* Vol. xxii. p. 470.

just the same as Mr. Park Harrison, viz. a more elevated temperature about new moon; this is also the opinion of the observers who accompanied the late polar expeditions.

The whole series gives, however, a sum in favour of the full moon, just as it ought to be.

You will agree with me that *if the new moon really gives a more elevated temperature at the surface of the earth than the full moon, the question is still more interesting*, because the opposite *ought* to be the case, since the whole earth necessarily contains more heat about the time of full moon than about the time of new moon. I have thought, at the same time, whether the moon, when it has north declination, imparts a more elevated temperature to our thermometer than when it has south declination; and I found it to be so; but the difference is equally variable and small.

Now we all admit that other perturbations, a thousand times as great, may totally eclipse for a considerable time the small influence of the moon; but nevertheless it seems to me, when we have 135 years of continued observation, that those perturbations ought to have annihilated themselves.

Why is the mean result so small and uncertain, while we are so certain of the heating action of the moon? The explanation that has occurred to me is this:—

The moon acts partly directly, and by that part the thermometers are most elevated the second or third day after full moon (the day of the greatest north declination for our latitude); but it also acts in some measure indirectly, by elevating the temperature of the aqueous vapour in the atmosphere. By the latter action the clouds and the air have a more elevated temperature, and contain more heat, about and after full moon; but some days must elapse before those particles of air and cloud reach the surface of the earth. When fourteen days elapse before they reach the surface, they have an action wholly opposed to the direct action, and thus the two parts of the action of the moon are necessarily neutralizing each other, more or less.

Hence it is that experiments at great elevations, such as those of Prof. Piazzi Smyth at the Peak of Teneriffe, are of the greatest value. It is one instance among many where they are indispensable.

My letter is already too long to give you my numbers. I will publish them, but would like very much to know your opinion before doing so. Probably you have not time and leisure just now to give your attention to this question. Perhaps you may think fit, however, to publish this letter of mine, with verbal correction, in the Philosophical Magazine, together with a few remarks of your own.

Excuse me for thus applying to you; it is in order to fix your attention on the subject and to be instructed by your opinion.

I am,

Most respectfully yours,

Buys Ballot.

[In the autumn of 1861 I swept the heavens on several days and

nights with my thermo-electric pile, and satisfied myself that this instrument is eminently fitted to examine "the mixed action of our atmosphere and stellar space," as regards radiation. My observations are referred to in the Philosophical Magazine, vol. xxii. p. 470. I still preserve my little observatory on the roof of the Royal Institution, in the hope of being able to carry out the idea expressed in a note at the page referred to. Until this has been done, any opinion of mine on the questions raised by my distinguished correspondent M. Buys Ballot, would be of little value; but I am glad to see the subject in such excellent hands. It also gives me pleasure to find that M. Poey has recently turned the thermo-electric pile to account in examining the atmosphere of Havana. This zealous observer thinks that his observations regarding the action of aqueous vapour are diametrically opposed to mine: I, however, entertain a strong hope that M. Poey himself will eventually clear up the apparent difference. The experiments demonstrative of the action of aqueous vapour on radiant heat of low refrangibility are, in my opinion, perfectly conclusive.—John Tyndall.]

ON CALORESCENCE.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

While the changing of the ultra-red rays of the spectrum of the electric light into a complete luminous spectrum agrees so far with the phenomena of fluorescence, that in both cases the change of period is wrought by the intermediation of ponderable substances which alter the rate of oscillation, still the fact that the one class of effects is produced in bodies at ordinary temperatures, while the other class requires the temperature of incandescence for their production, introduces a marked distinction between them. Instead, therefore, of the two words "negative fluorescence" provisionally adopted by me in an article in the January Number of the Philosophical Magazine, I would propose the word *Calorescence* to express the rendering of bodies luminous by the heat of the ultra-red rays.

John Tyndall.

A LETTER FROM JOHN DAVY, M.D., F.R.S., ADDRESSED TO THE EDITORS OF THE PHILOSOPHICAL MAGAZINE, IN CONTINUATION OF A FORMER ONE, AND ACCOMPANIED BY DOCUMENTARY EVIDENCE CONTRIBUTED BY SIR JAMES SOUTH, F.R.S., AND BENJAMIN GOMPERTZ, ESQ., F.R.S., IN REPLY TO A CERTAIN CHARGE MADE BY CHARLES BABBAGE, ESQ., F.R.S., AGAINST THE LATE SIR HUMPHRY DAVY WHEN PRESIDENT OF THE ROYAL SOCIETY.

Gentlemen,

Referring to my letter to you of the 20th October, published in the Philosophical Magazine for December in reply to certain

charges brought against the late Sir Humphry Davy, when President of the Royal Society, and also to the letter addressed to you by Sir John Herschel, which appeared in the same Magazine for January, partly in answer to mine, but less clear and satisfactory than I had expected, may I beg the favour of you to allow the following correspondence, in further vindication of the character of my brother, to have a place in the next Number of your periodical? A sense of duty impels me to make this request, and I trust that a sense of justice will induce you to comply with it.

I am, Gentlemen,

Lesketh How, Ambleside, January 19, 1865.

Your obedient Servant,
John Davy.

Dear Sir James,—I have to request your attention to a letter addressed by Sir John Herschel to "The Editors of the Philosophical Magazine and Journal," and published in the Philosophical Magazine of this month.

As you were present at the Council of the Royal Society, held on the 23rd November, 1826, I shall feel obliged if you will acquaint me whether you consider the statements contained in that letter to be perfectly accurate.

I am, yours faithfully,

Lesketh How, near Ambleside, January 3, 1865.

John Davy.

Letter from Sir James South, F.R.S., *to* Dr. John Davy, F.R.S.

The Observatory, Campden Hill, Kensington, January 11, 1865.

Dear Dr. John Davy,—Before I received your letter to me of the 3rd inst., my attention had been directed to Sir John Herschel's letter in the Philosophical Magazine of this month.

I can, dear Dr. John Davy, assure you it was with great surprise I read the representations of Mr. Babbage and Sir John Herschel, in reference to the Secretaryship of the Royal Society in 1826, because I knew them to be *painfully* inaccurate.

Sir John Herschel, Mr. Gompertz, and I are the only members *now* alive who constituted the Council of the Royal Society on November 23, 1826, and I therefore, immediately after my perusal of Sir John Herschel's letter, communicated in the following letter with Mr. Gompertz, in order to ascertain what he knew upon the subject.

Observatory, Kensington, January 2, 1865.

My dear Mr. Gompertz,—May I be permitted to direct your attention to the accompanying passages marked in red ink, contained in Mr. Babbage's work entitled "Passages from the Life of a Philosopher," and in the Philosophical Magazine of December 1864 and January 1865?

It is stated by Mr. Babbage (p. 186), that at the Council of the Royal Society, held Nov. 23rd, 1826, Dr. Wollaston, in reference to the Secretaryship, "asked Sir Humphry Davy if he claimed the

nomination as a right of the President, to which Sir Humphry Davy replied, that he did; and then nominated Mr. Children."

It is likewise stated by Sir John Herschel (Phil. Mag. Jan. 1865, p. 78), that "the President was distinctly asked by Dr. Wollaston [at the Council of the Royal Society, Nov. 23, 1826] whether he intended to use the privilege, by courtesy accorded to him, of naming the Secretary, to which no one would object, or fairly to throw it open to the Council. His answer to the former part of the alternative was susceptible of any sense that one might choose to put upon it; to the latter, it was both in Dr. Wollaston's opinion and my own a negative.'

You and I were present at that Council from the commencement to its end.

May I be permitted to ask you whether you remember having heard any conversation such as that which has been stated by Sir John Herschel and Mr. Babbage?

I assert, that no such conversation took place.

May I also be permitted to ask you, as a then member of the Council, whether you ever heard it even *surmised*, that Sir Humphry Davy had promised to appoint Mr. Babbage to the office of Secretary of the Royal Society, and had then violated that promise?

I am, my dear Mr. Gompertz,
Yours sincerely,

Benjamin Gompertz, Esq., F.R.S. J. South.

My respected old friend lost not a moment in replying to it, as follows:—

Kennington Terrace, Vauxhall,
January 2, 1865.

My dear Sir James,—In reply to your letter of the 2nd inst., I beg to state that I never heard the conversation alluded to by Sir John Herschel and Mr. Babbage, at the Council of the Royal Society, on November 23, 1826; and I never heard that Sir Humphry Davy had promised the Secretaryship to Mr. Babbage.

I write this with the wish that, if you think it necessary, you should publish it. Yours very sincerely,

To Sir James South, F.R.S. Benjn. Gompertz.

There are, however, some additional facts which in my opinion establish that your late brother, Sir Humphry Davy, did *not*, either directly or by *implication*, promise that he would appoint Mr. Babbage to the Secretaryship of the Royal Society, and consequently that Sir Humphry was not guilty of the *mala fides* of which he has been so recklessly accused.

On November 14, 1826 (the Council of the Royal Society was held on the 23rd of November), I received a letter from Sir John Herschel (then Mr. Herschel), of which the following is a copy:—

Sloane Street,
Tuesday, Nov. 14, 1826.

Dear South,—I am sorry not to have met with you, as I wanted to have had some talk with you about the Secretaryship of the Royal

Society. I have endeavoured to ascertain Babbage's sentiments upon the subject, and I think I see them pretty distinctly, and that in such light as makes me very desirous to have some personal communication with you about it.

Knowing that you yourself have formed something of a wish respecting it, I think you will be enabled to judge how high an idea I have formed of what may be expected from a disposition like yours, when I tell you that the object of my calling was to induce you yourself to name *him* to our President as a proper person to fill the office, if he has not already his eye on him.

My reasons I will explain *vivâ voce*; and if you do not see the force of them, at least you will have no cause to blame the feeling which gives them weight with me.

Yours very sincerely,
J. F. W. Herschel.

Immediately after the perusal of this letter, I went to the residence of Sir Humphry Davy (Park Street, Grosvenor Square), and was accompanied near to the house by Sir John Herschel, but who refused to accompany me into it. Not finding him (Sir Humphry) at home, I wrote a letter, of which the following is a copy, and left it for him on his library table:—

Nov. 14, 1826.

My dear Sir Humphry,—During many weeks I have not heard anything which gives me more pleasure to communicate, than the fact I have just arrived at, namely, that Mr. Babbage would probably accept the office of Secretary to the Royal Society, were you to offer it to him.

Most sincerely *praying* that nothing may have yet transpired to cause you to think of any one else, in which I feel sure every scientific member of the Society joins me,

I remain, trusting you will forgive the warmth of my expressions,

Yours very truly,
J. South.

In order *personally* to urge the claims of Mr. Babbage upon the consideration of Sir Humphry Davy, I called at his residence again in about half an hour after I had left the foregoing letter. Sir John Herschel walked with me near to the house, but he would not enter it and see Sir Humphry with me.

Sir Humphry was at home, he received me very kindly, and told me Herschel had given him information which had much surprised him, namely, that I would have accepted the Secretaryship if it had been offered to me; that he thought my never having given him the slightest idea I would take the office was not a friendly act towards him.

I replied, I had thought very little of the matter, but that, having met Mr. Babbage on Ludgate Hill a few days ago, I asked him if he would take the Secretaryship, and his reply was, "I would see the Society d——d first;" that I walked with him as far as Troughton's, and repeated my question, to which he returned the same answer;

that I then said, "If no one better can be found, I perhaps might take it, were it offered me, but I will not ask for it."

Sir Humphry Davy then said to me, "If you will *now* take the Secretaryship, no one else shall have it; to which I replied, "My dear Sir Humphry, the thing is impossible. In the letter that you have in your hand I have solicited you to give the appointment to Mr. Babbage, and I would cut my right hand off sooner than you and Wollaston should say of me, as both of you said of . . . . . , that 'he went to . . . . . to get the Clerkship of the Irons for Children, but that he came back, having secured it for himself!'"

Sir Humphry then said, "I have not given the place to Children, nor have I actually promised it to him, but I WILL NOT GIVE IT TO BABBAGE."

Under the foregoing facts and circumstances, it would be an unjustifiable encroachment upon your time, if I were at greater length to trespass upon your attention, except it be to remark—

> "Facilis descensus Averni;
> Sed revocare gradum, superasque evadere ad auras,
> Hic labor, hoc opus est."

I am, dear Dr. John Davy,
Yours very faithfully,
J. SOUTH.

P.S.—You have my permission to publish this letter.—J. S.

SIMPLE METHOD OF PREPARING THALLIUM. BY R. BUNSEN.

In a large sulphate-of-zinc works at Goslar, a lye is evaporated, obtained from the Rammelsberg pyrites which is so rich in thallium that pounds of this metal can be readily obtained from it. It contains, besides other constituents, 21·74 per cent. of sulphate of zinc, 0·536 of sulphate of cadmium, 0·05 of chloride of thallium, and 0·28 of sulphate of copper. Mixed with an equal volume of hydrochloric acid, an immediate precipitate of chloride of thallium is obtained. When iodide of potassium is added to it in the presence of an adequate quantity of hyposulphite of soda, iodide of thallium is precipitated, mixed, however, with some subiodide of copper if the quantity of hyposulphite is insufficient. But as the addition of these substances hinders the production of white vitriol, I have found a way which is not only simpler, but which also introduces an essential improvement in the manufacture. It consists in placing in the cold lye sheets of zinc, by which copper, cadmium, and thallium are precipitated. In this way, from a cubic metre of the lye, for 7·4 kilogs. of zinc, 6·4 kilogs. of a spongy metallic precipitate were obtained, which was rapidly washed out in a flannel bag. Besides some lead and zinc, this contained 4·2 kilogs. of cadmium, 1·6 kilog. of copper, and 0·6 kilog. of thallium. When digested with water to which some sulphuric acid was added from time to time, the cadmium and thallium dissolved with disengagement of hydrogen, leaving the copper. To the sulphuric acid solution, 0·5 kilog. of iodide of potassium were added, which precipitated 0·97 kilog. of chemically pure iodide of thallium, which was readily washed by decantation. —Liebig's *Annalen*, January 1865.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

MARCH 1865.

XXVI. *On the Elastic Force of Steam of Maximum Density; with a new Formula for the expression of such force in terms of the Temperature.* By THOMAS ROWE EDMONDS, *B.A. Cantab.**

THE laws of steam and other vapours, in relation to elastic force, density, quantity of heat, and temperature, possess claims on the attention of mankind which are not surpassed in interest or importance by the claims of any other branch of physical science. The foundation for the knowledge of these laws must be sought in observations and experiments of great variety, extent, and accuracy. The best of such observations (as is generally acknowledged) are those of M. Regnault, made by order and at the expense of the Government of France. The report of M. Regnault to the Government of his nation was made in the year 1847. His Report fills the twenty-first volume of the *Mémoires de l'Académie des Sciences de l'Institut de France*. It is divided into ten memoirs on steam and allied subjects. The eighth memoir, entitled "Elastic Forces of Steam," contains the materials now presented for consideration.

Among the laws of steam desired to be known, the most important has generally been acknowledged to be the law which indicates the relation between temperature and elastic force of steam confined with water in a closed vessel—such as the boiler of a steam-engine. On the application of heat to such a vessel, the enclosed water will continually diminish by conversion into steam, and the enclosed steam will continually increase in density and elastic force until the point of condensation or saturation is attained, or, which is the same thing, until the maximum den-

* Communicated by the Author.

sity appropriate to the given temperature is attained. The elastic force of such steam is measured by the height of a column of mercury contained in a tube of which the open end is inserted in the boiler containing both water and steam. At a given temperature, when the elastic force ceases to increase, the contained steam must be of maximum density. It is to be noted that when, by increasing the temperature, the whole of the contained water has been converted into steam, any further addition to the temperature cannot further increase the density of the steam, which then becomes "dry" or "superheated" steam, a kind of steam which is not here taken into consideration.

The problem offered for solution is this:—Given the temperature of the water and steam contained in the boiler, it is required to find, either by experiment or calculation, the corresponding elastic force when such force is constant and at its maximum. This problem has been completely solved by M. Regnault so far as experiment is concerned, and this for all ordinary ranges of temperature and pressure—extending from 130° Centigrade *above* to 130° Centigrade *below* the boiling-point of water, or from a pressure of twenty-seven atmospheres to a pressure of only the two-thousandth part of one atmosphere. What remains to be desired is, the knowledge of the law which connects together the experimental results, and which will enable a person to determine by calculation the elastic force at every other temperature from the known elastic force at any one temperature.

At different times during the last fifty years there have appeared various empirical formulæ intended to express approximately the elastic force of steam of maximum density in terms of the temperature. The comparative merits of several of these formulæ have been discussed by the two commissions (on steam, &c.) appointed by the French Government, viz. by MM. Dulong and Arago in their Report of the year 1829, and by M. Regnault in his Report made in the year 1847. The conclusion arrived at by M. Regnault is, that one only of these empirical formulæ yields results sufficiently near the truth to render it capable of being usefully applied to purposes of interpolation. The favoured formula is well known as that of M. Roche, who was formerly professor in a college or military school at Toulon. It was published in 1829, or before that year. This formula, when reduced to its simplest terms by making $P=1$ when $t=0$ (the former quantity representing *elastic force*, and the latter *temperature* measured on the thermometric scale), is the following:

$$\log P_t = A \frac{t}{1+Mt},$$

which formula, when expressed in series, becomes

$$\log P_t = At\{1 - Mt + M^2t^2 - M^3t^3 + \&c.\}.$$

The earliest of the empirical formulæ referred to is that of our own countryman Dalton, who suggested, as the foundation of useful formulæ of interpolation, the principle, that the elastic forces increase in geometrical progression, whilst the temperatures increase in arithmetical progression. Thus Dalton's formula of interpolation would be

$$\log P_t = At,$$

a formula consisting of one term only, and that term being the first of the series (above given) constituting the Roche formula.

M. Regnault (at page 587 of the volume* cited) makes a valuable remark which is applicable to the two empirical formulæ just mentioned, as well as to other empirical formulæ relating to steam. The remark is, that such formulæ express correctly only the first, or only the first and second terms of the series constituting the true formulæ. That is to say (applying and extending the principle), if the true formula, expressed in series of ascending powers of t, were

$$\log P_t = At + Bt^2 + Ct^3 + \&c.,$$

then the approximate formula of the first degree would be $\log P_t = At$, which is that of Dalton. And the approximate formula of the second degree would be $\log P_t = At + Bt^2 + \&c.$, of which Roche's formula is an example, the first and second coefficients A and B being identical with the first and second coefficients of the formula expressing the true law. Proceeding in the same course, it may safely be said that if an approximate formula of the third degree were discovered, it is highly probable that such formula would coincide with the true formula, not only in the first three terms, but in all the succeeding terms of the two series, and that such formula would be as great an improvement on the Roche formula as the latter is an improvement on the formula of Dalton.

The general character of the law of variation according to temperature of the elastic force of steam of maximum density, may be easily perceived on inspection of the tabulated values of these forces as given either by MM. Arago and Dulong or by M. Regnault. If the logarithms of these forces, separated by equal intervals of temperature (as 5 or 10 degrees Centigrade), be written under one another in a vertical column, and the differences of the consecutive logarithms be taken, the series thus formed of the quantities $\Delta \log P$ will evidently be composed of terms uniformly decreasing with the temperature, from the very

* *Mémoires de l'Académie*, &c., vol. xxi.

low to the very high temperatures. It will be readily seen that, for short ranges of temperature, the successive values of $\Delta \log P$ are in near agreement with Dalton's law, and that for moderate ranges the values of $\Delta \log P$ are in near agreement with Roche's law. The result of the inspection (if carried to a sufficient degree of minuteness) can hardly fail to be the conviction that there exists a fixed and simple law of connexion between the force and temperature, and that Roche's law is only an approximation (and not a very close one) to such fixed and simple law.

The manifest uniformity of decrease with temperature of the finite differences of the logarithms of the observed elastic forces of steam of maximum density is indicative of similar uniformity of decrease of the differential or indefinitely small differences of these logarithms. If the elastic force (P) be a function of the temperature (t), and if the finite differences ($\Delta \log P$) are known in terms of the temperature, then the differentials ($d \cdot \log P$) are similarly known; and conversely. The existence of an exact law being assumed, the simplest expression of that law will be contained in the differential ($d \cdot \log P$) in terms of the temperature (t). If such differential can be obtained from the observations of M. Regnault, and if such differential is of a simple form and readily integrable, the law which governs the elastic forces at all temperatures will be ascertained.

The Table given by Regnault as the final result of his observations on the elastic forces of steam is found at page 624 of the volume cited. In this Table the forces are stated in millimetres of mercury for every degree Centigrade, from 32° C. below the freezing-point of water to 230° C. above that point. At the temperature 100° C., taken as the boiling-point of water, the force is 760 millims., which is taken to represent the pressure of one atmosphere. There is also given in the same Table a column of differences or increments of forces for all consecutive intervals of one degree of temperature. By means of this column the value of the rate of increment of the force P, and consequently the value of $d \cdot \log P$, may be easily obtained for any required temperature.

The differential of hyperbolic logarithm P is $\frac{dP}{P}$, which is the limit of $\frac{\Delta P}{P}$ when the intervals of temperature are indefinitely diminished. By taking ΔP equal to half the sum of the increment preceding and the increment following the value of P for a given temperature, and then dividing by P, we get the mean value of the rate of increment per degree $\left(\frac{\Delta P}{P} \text{ or } \frac{P_{+1} - P_{-1}}{2P_0}\right)$ for the interval of two degrees, one of which precedes and the other follows the

given temperature. When the intervals taken are small (as one degree Centigrade), such mean value coincides exactly, so far as observations are concerned, with the rate of increment per degree of the force existing at the middle point of the interval or at the given temperature. Such rate of increment per degree is the hyperbolic logarithm α taken to represent $\frac{\Delta P}{P}$.

According to the known properties of hyperbolic logarithms, when the rate of increment of force is constant throughout any interval (as one degree taken as a unit), and when such interval is divided into an infinite number (q) of equal parts, then we have for the constant ratio of increase of force for such infinitely small interval $\left(1+\frac{\alpha}{q}\right)=e^{\frac{\alpha}{q}}$, and consequently for the total unit interval containing q such parts,

$$\left(1+\frac{\alpha}{q}\right)^q=e^{\frac{\alpha}{q}\times q}=e^{\alpha}=1+r,$$

if it is known that the force unity under a constant rate of increment becomes $(1+r)$ at the end of the unit interval. From the above equation we obtain the following two values of α in terms of r:—

$$\alpha=\text{com log}\,(1+r)\times\frac{1}{k},$$

and, by expressing hyperbolic logarithm in terms of its corresponding number,

$$\alpha=r-\frac{r^2}{2}+\frac{r^3}{3}-\&c.$$

In the above equations the indefinitely small quantity $\frac{\alpha}{q}$ represents

$$d.\log P=\frac{dP}{P}=\frac{\Delta P}{P}\times\frac{1}{q}.$$

The values of $\frac{\Delta P}{P}$, or the rates of increment of force P for every tenth degree of temperature, commencing with 30° C. below the freezing-point of water, have been obtained in the manner above described from M. Regnault's principal and adopted Table, and are contained in the last column of Table I. hereunto annexed. The ratios of these quantities $\left(\frac{\Delta P}{P}\right)$ to one another are the same (or very nearly the same), at the same temperatures, as the ratios of the differentials ($d.\log P$). On comparing together these ratios $\frac{\Delta P}{P}$, it is found that they all bear a

very simple relation to one another, which is that of $\left(\frac{1}{a}\right)^{\frac{1}{k}}$ to $\left(\frac{1}{a+t}\right)^{\frac{1}{k}}$. The quantities a and $a+t$ represent temperatures measured from an ideal fixed point which has been called the absolute zero of temperature. The quantity a represents the absolute temperature of the point adopted as the zero of the thermometrical scale in use. The quantity t represents the number of degrees in either direction measured on such scale. The exponent $\frac{1}{k}$ is the hyperbolic logarithm of 10, and is equal to 2·302585.

From what has preceded, it ensues that if α be the hyperbolic logarithm representing the ratio of increment of the elastic force P at the absolute temperature a, then the hyperbolic logarithm representing the rate of increment of elastic force at any other absolute temperature $(a+t)$ will be

$$\alpha \times \left(\frac{a}{a+t}\right)^{\frac{1}{k}} = \alpha \left(1+\frac{t}{a}\right)^{-\frac{1}{k}}.$$

Using differentials, we may say that if $\frac{d\mathrm{P}}{\mathrm{P}_0} = \frac{\alpha}{q}$, then will

$$d \,.\, \log \mathrm{P}_t = \frac{\alpha}{q}\left(1+\frac{t}{a}\right)^{-\frac{1}{k}} = \alpha \left(1+\frac{t}{a}\right)^{-\frac{1}{k}} dt.$$

The above differential of $\log \mathrm{P}_t$ yields on integration the equation following, by which is expressed in logarithms the ratio of elastic force of steam of maximum density at any thermometric temperature t to the elastic force at absolute temperature a, whence t is measured;

$$\mathrm{hyp\ log}\, \mathrm{P}_t = \frac{\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}.$$

The quantity n is put for $\left(\frac{1}{k}-1\right) = 1{\cdot}302585$. The above equation, otherwise expressed, is

$$\mathrm{P}_t = e^{\frac{\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}} = 10^{\frac{k\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}}.$$

The quantity e is the base of the hyperbolic system of logarithms, and is equal to 2·7182818, and k is the modulus of the common or decimal system of logarithms, and equal to ·4342945.

By means of the formulæ above given, the values of (P_t) the elastic force, and (α_t) the rate of increment per degree of such

force, have been calculated for all temperatures differing by intervals of 10° C., beginning at the absolute temperature 246°, and ending at the absolute temperature 506°. These values are exhibited in Table I. annexed hereto. They are placed side by side with similar values for the same temperatures, which have been deduced directly from M. Regnault's principal Table printed at page 624 of vol. xxi. of the *Mémoires de l'Académie*, &c. On inspection and comparison, it will be seen that the two different series of results are in close coincidence with one another in nearly all parts of the scale. The only exception worthy of note occurs for temperatures below +10° C. But in this part of M. Regnault's Table the elastic forces, as well as the increments of elastic forces, have lost the regularity and uniformity of progressive increase or decrease which is remarkable at all temperatures above 10° C. On account of the want of uniformity at this part of the scale, very little weight can be attached to the slight discrepancies here existing between M. Regnault's adjusted Table and my theoretical Table. The errors of observation amount apparently to about one part in one thousand for any temperature *above* 10° C., and about one part in one hundred for temperatures *below* that point. The extreme error (when the two compared at the same temperature are in opposite directions) might thus amount to one in five hundred at temperatures *above*, and one in fifty at temperatures *below* 10° C. It is only within these limits that the present new theoretical Table differs from M. Regnault's adjusted Table and the selected observations adopted by him.

M. Regnault informs his readers (page 589) that, in the adjustment of his chief Table, all his definitive calculations have been executed by using only the formula of M. Roche or that of M. Biot for purposes of interpolation. The formula of M. Biot is this:

$$\log P = a + l\alpha^t + c\beta^t,$$

the five constants contained requiring for their determination five observed values of P separated from one another by equal intervals of temperature. M. Regnault afterwards says (page 599) that the formula of interpolation (E) which he has used for all temperatures below the freezing-point of water, or 0° C., is $P = a + b\alpha^t$, which formula is a modification of Biot's general formula, obtained by cutting off the third term and substituting P for log P. At page 598 M. Regnault states that he has used another formula (D), wherein log P, for all temperatures between 0° and 100° C., is represented by the general formula of Biot. At page 600 it is stated that another formula (F), wherein log P is represented by the general formula of Biot, has been used for all interpolations between 100° C. and 230° C. It thus appears

that M. Regnault's adjusted Table is a composite Table consisting of three separate series, each regulated by its specific constants, whether three or five in number. When this mode of construction is taken into account, it will not be expected that such a Table will be in complete harmony with a theoretical Table deduced from one uniform law throughout the whole range of temperature observed. Disagreement between Tables so differently constructed is most likely to occur at the temperatures 0° and 100° C., the points at which the different series of Regnault are intended to join one another.

At page 606, however, M. Regnault gives another formula (H) according to Biot, with five special constants, which formula is intended to be applied generally to the whole range of temperature observed, and to prove to the reader that the single formula (H), with its five special constants, will yield nearly the same results as those contained in the principal or adjusted Table of M. Regnault, and obtained by three sets of constants, amounting to thirteen in number. The greatest proportional difference between the elastic forces of steam, as given by Regnault's adjusted Table and by my new theoretical Table, occurs at the temperature 0° C., or at the temperature of melting ice. But the elastic force at the same temperature given by the general formula (H) already described, differs from the result of my formula in an opposite direction. The elastic force at temperature 0° C. is, in millimetres, 4·60 according to Regnault's adjusted Table; in my theoretical Table it is 4·52 millims.; whilst by the general formula (H) it is 4·48 millims. This will be seen on reference to Table II. hereunto annexed. In the same Table II. is included a column (extracted from page 608) of elastic forces, at intervals of 10° C., measured on a copper engraving which contains a geometrical construction of the results of M. Regnault's observations, and which represents by a continuous curve the elastic forces observed at every temperature, or which represents rather those observations which were thought most worthy of being relied upon. The values exhibited according to this "graphic curve" (as it is designated by M. Regnault) are of doubtful authority in respect of the small deviations at temperatures below 10° C. from the law now offered as the true law; for it is not improbable that M. Regnault, biassed by his favourable opinion of the formula of Roche, has selected for adoption those experiments which were most in harmony with that erroneous formula.

The general formula above given for the elastic force in terms of the temperature may, by expanding the exponential $\left(1+\frac{t}{a}\right)^{-n}$ by the binomial theorem, be expressed in the series following:

$$\text{com log } P_t = k\alpha t \left\{ 1 - \frac{n+1}{2}\frac{t}{a} + \frac{\overline{n+1}}{2}\cdot\frac{\overline{n+2}}{3}\frac{t^2}{a^2} - \frac{\overline{n+1}}{2}\frac{\overline{n+2}}{3}\frac{\overline{n+3}}{4}\frac{t^3}{a^3} + \&c. \right\}$$

$$= k\alpha t \left\{ 1 - 1{\cdot}151292\frac{t}{a} + 1{\cdot}267414\frac{t^2}{a^2} - 1{\cdot}363290\frac{t^3}{a^3} + \&c. \right\}.$$

The above series, representing the true law of elastic force of steam, may be used to show in what manner Roche's law and two other approximate laws may be formed. According to the true law, and also according to the three approximate laws, the common multiplier of all the terms of the four different series is $k\alpha t$—the quantity α being the hyperbolic logarithm representing the rate of increase per degree when the thermometric temperature is 0 or dt. Also the true law as well as the three approximate laws agree in having their two first terms the same, $k\alpha t\left(1 - \frac{n+1}{2}\frac{t}{a}\right)$. It is at the third term only that the variations begin.

The three approximate formulæ are these:—

$$\log P_t = k\alpha t \left(1 + \frac{t}{a}\right)^{-\frac{n+1}{2}} \text{ (No. 1)},$$

$$\log P_t = k\alpha t \left(1 + \frac{n+1}{2}\frac{t}{a}\right)^{-1} \text{ (No. 2, or Roche's formula)},$$

$$\log P_t = k\alpha t e^{-\frac{n+1}{2}\frac{t}{a}} \text{ (No. 3)}.$$

Putting $\frac{n+1}{2}$ or $\frac{1}{2k} = m$, we shall have for the three approximate formulæ for $\log P_t$,

$$k\alpha t\left(1 + \frac{t}{a}\right)^{-m}, \quad k\alpha t\left(1 + m\frac{t}{a}\right)^{-1}, \quad \text{and} \quad k\alpha t e^{-m\frac{t}{a}}.$$

The coefficients of the third and fourth terms of the above three approximate formulæ are, omitting the common multiplier $k\alpha$, as follows:—

$$+1{\cdot}238383 - 1{\cdot}300835 \text{ (No. 1)},$$
$$+1{\cdot}325473 - 1{\cdot}526007 \text{ (No. 2)},$$
$$+ \cdot662736 - \cdot252668 \text{ (No. 3)}.$$

The coefficient of the third term of the true formula being 1·267414, it will be seen that the coefficient of the same term in Roche's formula is ·058059 *in excess*, and in formula No. 1 is ·029031 *in defect*. That is, Roche's approximate formula

deviates from the true formula at the third term exactly twice as much as approximate formula No. 1 does.

The Roche formula, as given above, differs considerably from the Roche formula used by M. Regnault. In the correct formula, $A = k\alpha$ and $M = \frac{m}{a}$ are constant for all temperatures, beginning with $t = 0$. In the modified formula used by M. Regnault and others, A and M are made to vary (in a small degree) according as t varies. For example, in the pure Roche formula, where P is taken equal to unity at $-20°$ C., the value of A or $k\alpha$ is ·037679, and the value of M or $\frac{m}{a}$ is ·0044973. In the mixed Roche formula, constituted by changing A and M, in order to force the formula to give true values of P at 100° C. and at 220° C. (as M. Regnault has done for the Roche Table, contained in column 5 of Table II. hereunto annexed), A is changed to ·038232, and M to ·004774. The consequence of these alterations of the constants belonging to the real curve at its origin is to produce a fictitious curve of great irregularity, more especially at points near those of forced coincidence with a real curve belonging to an equation whose constants are really constants. As an instance of the defect of the Roche formula constituted as above, I may mention that the true elastic forces (yielded by the new formula) at temperatures $-20°$ C. and 0° C. are ·92 and 4·52 millims. respectively, whilst the elastic forces at the same temperatures given by the Roche formula constituted as above are ·92 and 4·59. The error thus found to exist in the Roche formula is $4·59 - 4·52$, or ·07 millim. at the temperature 0° C., when $-20°$ C. is taken as the point of departure. This error is a near representation of the whole of the discrepancy, at temperature 0° C., between the forces given by the new formula and by M. Regnault in his adjusted Table.

A comparison may be usefully instituted between the laws of variation with temperature of the elastic force (P) of steam of maximum density and of the elastic force (p) of a perfectly elastic gas or vapour of constant density. It is generally acknowledged that the equation $p = 1 + \frac{t}{a}$ represents the elastic force of a perfectly elastic gas of constant volume and density when raised to the temperature t, compared with a unit of elastic force possessed at temperature 0°. We have consequently, on differentiating,

$$d \cdot \log p = \frac{dp}{p} = \frac{\frac{dt}{a}}{1 + \frac{t}{a}}.$$

It has already been shown that $d.\log \mathrm{P} = \alpha \dfrac{dt}{\left(1+\frac{t}{a}\right)^{\frac{1}{k}}}$. On dividing the latter equation by the former, we have

$$\frac{d.\log \mathrm{P}}{d.\log p} = \alpha a\left(1+\frac{t}{a}\right)^{-n} = \alpha a p^{-n} = \alpha a e^{-nt_1};$$

on putting $p = 1+\frac{t}{a} = e^{t_1}$ so that $t_1 = \mathrm{hyp}\log p$, then

$$\frac{d.\log \mathrm{P}}{dt_1} = \alpha a e^{-nt_1} \text{ and } d.\log \mathrm{P} = \alpha a e^{-nt_1}\, dt;$$

integrating, we get

$$\log \mathrm{P} = \text{constant} - \frac{\alpha a}{n} e^{-nt_1} = \frac{\alpha a}{n}\left\{1 - e^{-nt_1}\right\},$$

which agrees with the general equation first obtained.

It has been shown that $d.\log \mathrm{P}$ varies as $\left(1+\frac{t}{a}\right)^{-\frac{1}{k}}$, and $d.\log p$ as $\left(1+\frac{t}{a}\right)^{-1}$. The quantity a, which is common to both expressions, is the most important constant in the laws of steam and other vapours, and probably in the laws of liquids and solids also. At the zero of the Centigrade thermometer (the temperature of melting ice) a is equal to 276 Centigrade degrees. This number represents the absolute temperature of melting ice, measured from an ideal fixed point at which all heat disappears. This number approaches very near to the reciprocal of the coefficient of expansion of gases whose elasticity is nearly perfect, and probably would be found exactly equal to such reciprocal if a gas of perfect elasticity could be found. At the zero of the Centigrade thermometer, in atmospheric air of the ordinary density for that temperature, the coefficient of expansion per degree is $\cdot 003665 = \dfrac{1}{272{\cdot}9}$, nearly as determined by M. Regnault (page 73). According to the same authority (page 120), the coefficients of expansion of different gases approach nearer to equality with one another according as their densities diminish; and he believes that it is not improbable that the supposed law, that all gases have the same coefficient of expansion, may be true when their densities are indefinitely diminished. In air of the temperature of melting ice and of density 4·810, he found (page 110) the coefficient of expansion to be $\dfrac{1}{269{\cdot}6}$; and in air of density 0·144 to be $\dfrac{1}{274{\cdot}4}$. According to these two experiments, there is a difference of 4°·8 C. in

the place of the absolute zero of temperature in respect of densities of air in the proportion of 33 to 1. If a further reduction in the density of the air in a similar proportion could have been effected, it is not improbable that a coefficient of expansion equal to $\frac{1}{276}$ nearly would have been obtained, which coefficient of expansion is the constant quantity $\left(\frac{1}{a}\right.$ for temperature 0° C.$\left.\right)$ involved in all M. Regnault's experimental results on the elastic forces of steam of maximum density at different temperatures.

When the value of the constant a has been determined for any fixed temperature, the only constant remaining to be determined is α in the general formula for the ratios of elastic forces at all temperatures. As has been said, this quantity α is the hyperbolic logarithm representing the increment per degree of the elastic force at the precise absolute temperature $a°$ of the point fixed as the origin of thermometrical temperature $t°$. In the construction of my theoretical Table, I have found it most convenient to adopt 100° C. as the origin of the thermometrical scale, so that the value of a in this case is 376°, being 100° added to 276° when the latter is the value of a for the temperature of melting ice. The whole range of temperature observed by M. Regnault is comprised between 130° above the temperature of 100° C., and 130° below that temperature. Consequently if $t°$ be reckoned from 100° C., all thermometrical temperatures referred to may be expressed in terms of the absolute temperature $(a+t)$ or $(a-t)$, wherein $a=376$, and t is any quantity not exceeding 130° C. The lowest absolute temperature observed is 246°, and the highest 506°.

When the value of α has been obtained for any absolute temperature (a), the value of α_t for any temperature greater by $t°$ may be obtained from the equation $\alpha_t=\alpha_0\left(1+\frac{t}{a}\right)^{-\frac{1}{k}}$. The value which I have adopted for α at 376° absolute temperature is ·03580. When this number is inserted in lieu of α in the general formula, we get, first, when t is positive,

$$\text{com log } P_t=\frac{k\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}=\frac{k\times\cdot0358\times376}{1\cdot302585}\left\{1-\left(1+\frac{t}{376}\right)^{-}\right\}$$

$$=4\cdot487960\left\{1-\left(1+\frac{t}{376}\right)^{-n}\right\}.$$

Secondly, when t is negative,

$$\text{com log } P_{-t}=-4\cdot487960\left\{\left(1-\frac{t}{376}\right)^{-n}-1\right\}.$$

By means of these two equations have been calculated the theoretical elastic forces given in all four of the Tables hereunto annexed.

On inspection of the last two columns of Table I. hereunto annexed, it will be seen that my theoretical values of α_t coincide at the same temperature almost exactly with the values of α_t directly obtained from the adjusted Table of M. Regnault for all temperatures above + 10 degrees of the Centigrade thermometer. It is, however, to be remarked that my adopted theoretical value of α for 100° C., which is ·0358, differs more from M. Regnault's experimental number at the same temperature than it ought to do if the smaller differences between theory and experiment existing before and after that temperature are to be taken as a guide. This discrepancy, however, is easily accounted for by the statement already made, that M. Regnault's adjusted Table is a composite Table formed in three sections, two of which touch one another, but do not unite together, at the temperature 100° C.

The general equation for the ratio of the elastic force at temperature $t°$ to elastic force at temperature 0° having been obtained, there remains to be added for practical purposes a common multiplier to take the place of unity previously used to express the elastic force at temperature 0°. The standard of measurement of force usually adopted is either the height or the weight of a vertical column of mercury which is in equilibrium with the ordinary pressure of the atmosphere. The height of the column of mercury adopted to represent one atmosphere is 760 millimetres, or 29·9218 English inches. The pressure or weight of such a vertical column of mercury amounts to 2116·4 pounds avoirdupois on the square foot, or to 14·7 pounds on the square inch. Either of the above four quantities may be called H, and used as a multiplier in the general equation for the ratios of P at different temperatures. The equation for practical use will then become

$$P_t = H \times e^{\frac{\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}} = H \times 10^{\frac{k\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}},$$

and using common logarithms,

$$\log P_t = \log H + \frac{k\alpha a}{n}\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}.$$

It is not difficult, though somewhat laborious, to calculate from the general formula the value of log P for every degree of temperature. The properties of the formula are, however, such that all the desired results can be obtained without incurring the labour of making a direct use of the formula, except for obtaining the values of log P for two temperatures only. If a

Table of five columns be formed, the first column denoting absolute temperatures from 246° to 506°, the second column denoting values of log P, the third column denoting values of Δ log P, the fourth column denoting values of log Δ log P, and the fifth column denoting values of Δ log Δ log P, it will be found that the quantities in the fifth column are the reciprocals of the numbers in the first column nearly. In other words, it is found that $\Delta \log \Delta \log P = \frac{1}{a+t}$ nearly. Even when intervals so great as 10° C. are taken, the differences are very small between the numbers in the fifth column and the reciprocals of the absolute temperature. In order to form by the use of logarithms a Table of elastic forces of steam of maximum density for intervals of one degree Centigrade, or one degree Fahrenheit, nothing is required beyond, 1st, a Table of reciprocals of numbers representing absolute temperatures; 2ndly, the value of Δ log P for any one specific interval of temperature; and 3rdly, the value of log P for any one specified temperature.

The remarkable property just mentioned is consequent on the form of the differential coefficient of log P_t, which is

$$\text{either } \frac{\alpha}{\left(1+\frac{t}{a}\right)^{\frac{1}{k}}} \text{ or } \alpha\left(1+\frac{t}{a}\right)^{-\frac{1}{k}}.$$

Calling this α_t, and taking the ratio of two consecutive coefficients at unit intervals, we get

$$\frac{\alpha_{t+1}}{\alpha_t} = \frac{d.\log P_{t+1}}{d.\log P_t} = \left(\frac{a+t+1}{a+t}\right)^{-\frac{1}{k}} = \left(1+\frac{1}{a+t}\right)^{-\frac{1}{k}},$$

which becomes, when the intervals of temperature are indefinitely diminished,

$$\frac{\alpha_{t+dt}}{\alpha_t} = \left(1+\frac{dt}{a+t}\right)^{-\frac{1}{k}} = e^{-\frac{dt}{a+t}\times\frac{1}{k}} = 10^{-\frac{dt}{a+t}}.$$

Taking common logarithms of both sides of the former of the above two equations, we get

$$\text{com log } \frac{\alpha_{t+1}}{\alpha_t} = -\frac{1}{k}, \text{ com log } \frac{a+t+1}{a+t},$$

or, which is the same thing,

$$(\text{com log } \alpha_{t+1} - \text{com log } \alpha_t) = -\big(\text{hyp log } (a+t+1) - \text{hyp log } (a+t)\big).$$

That is to say, the differences between the common logarithms of the rates of increment of the elastic forces are equal to the differences of the hyperbolic logarithms of the corresponding absolute temperatures. This is another way of expressing the essential principle involved in the law of steam of maximum density.

It may also be shown from the general formula, on putting $\Delta \log P_t = \log \frac{P_{t+1}}{P_t}$, that

$$\frac{\log P_{t+1} - \log P_t}{\log P_t - \log P_{t-1}} = \frac{1 - \frac{1}{k}\frac{x}{2}}{1 + \frac{1}{k}\frac{x}{2}} = e^{-\frac{1}{k}x} = 10^{-x} = 10^{-\frac{1}{a+t}} \text{ nearly.}$$

By taking the ratios of two consecutive differential coefficients of log P corresponding to the temperature $t - \frac{1}{2}$ and $t + \frac{1}{2}$, we get

$$\frac{d.\log P_{t+\frac{1}{2}}}{d.\log P_{t-\frac{1}{2}}} = \left(\frac{a+t+\frac{1}{2}}{a+t-\frac{1}{2}}\right)^{-\frac{1}{k}};$$

but

$$\frac{d.\log P_{t+\frac{1}{2}}}{d.\log P_{t-\frac{1}{2}}} = \frac{\Delta \log P_{t+1} - \Delta \log P_t}{\Delta \log P_t - \Delta \log P_{t-1}} \text{ very nearly;}$$

consequently

$$\frac{\Delta \log P_t}{\Delta \log P_{t-1}} = \left(\frac{a+t+\frac{1}{2}}{a+t-\frac{1}{2}}\right)^{-\frac{1}{k}},$$

the last being a formula which gives the ratio to one another of consecutive values of $\Delta \log P_t$ with great accuracy.

Equating the two values thus obtained of $\frac{\Delta \log P_t}{\Delta \log P_{t-1}}$, we get, on taking common logarithms,

$$-\frac{1}{a+t} = -\frac{1}{k} \operatorname{com\,log} \frac{a+t+\frac{1}{2}}{a+t-\frac{1}{2}}$$

$$= -\operatorname{hyp\,log} \frac{a+t+\frac{1}{2}}{a+t-\frac{1}{2}}.$$

If Q be taken to represent $\Delta \log P$, so that

$$Q_0 = \log \frac{P_1}{P_0}, \quad Q_1 = \log \frac{P_2}{P_1}, \quad Q_t = \log \frac{P_{t+1}}{P_t},$$

we shall have, according to what has preceded,

$$\frac{Q_t}{Q_{t-1}} = \frac{\log P_{t+1} - \log P_t}{\log P_t - \log P_{t-1}} = 10^{-\frac{1}{a+t}} = \left(\frac{a+t+\cdot 5}{a+t-\cdot 5}\right)^{-\frac{1}{k}}.$$

If, according to the former of the two equations, we take successive values of $\frac{Q_t}{Q_{t-1}}$ differing from one another by intervals of a unit of temperature, we have

$$\frac{Q_1}{Q_0} = 10^{-\frac{1}{a+1}}, \quad \frac{Q_2}{Q_1} = 10^{-\frac{1}{a+2}}, \quad \frac{Q^t}{Q_{t-1}} = 10^{-\frac{1}{a+t}}.$$

By multiplying together the quantities on the same side of these

equations we get

$$\frac{Q_1}{Q_0}\times\frac{Q_2}{Q_1}\times\frac{Q_3}{Q_2}-\frac{Q_t}{Q_{t-1}}=10^{-\left\{\frac{1}{a+1}+\frac{1}{a+2}+\&c.\frac{1}{a+t}\right\}},$$

or

$$\frac{Q_t}{Q_0}=10^{-\left(\frac{1}{a+1}+\frac{1}{a+2}+\&c.\frac{1}{a+t}\right)},$$

and, taking common logarithms of each side,

$$\text{com log}\frac{Q_t}{Q_0}=-\left\{\frac{1}{a+1}+\frac{1}{a+2}+\&c.\ \frac{1}{a+t}\right\}.$$

Now taking, according to the second of the above two equations, the value of $\frac{Q_t}{Q_{t-1}}$, we get, on substituting for t the numbers 1, 2, 3, and t in succession, and taking 10 as the unit of temperature,

$$\frac{Q_1}{Q_0}=\frac{\log\frac{P_2}{P_1}}{\log\frac{P_1}{P_0}}=\left(\frac{37{\cdot}6+1{\cdot}0+{\cdot}5}{37{\cdot}6+1{\cdot}0-{\cdot}5}\right)^{-\frac{1}{k}}=\left(\frac{39{\cdot}1}{38{\cdot}1}\right)^{-\frac{1}{k}},$$

$$\frac{Q_2}{Q_1}=\left(\frac{37{\cdot}6+2{\cdot}0+{\cdot}5}{37{\cdot}6+2{\cdot}0-{\cdot}5}\right)^{-\frac{1}{k}}=\left(\frac{40{\cdot}1}{39{\cdot}1}\right)^{-\frac{1}{k}},$$

$$\frac{Q_t}{Q_{t-1}}=\frac{\log\frac{P_{t+1}}{P_t}}{\log\frac{P_t}{P_{t-1}}}=\left(\frac{37{\cdot}6+t+{\cdot}5}{37{\cdot}6+t-{\cdot}5}\right)^{-\frac{1}{k}}=\left(\frac{38{\cdot}1+t}{37{\cdot}1+t}\right)^{-\frac{1}{k}}.$$

On multiplying together the quantities on the same sides of the equations, we have

$$\frac{Q_1}{Q_0}\times\frac{Q_2}{Q_1}\times\frac{Q_3}{Q_2}\times\ldots\frac{Q_t}{Q_{t-1}}=\left\{\frac{39{\cdot}1}{38{\cdot}1}\times\frac{40{\cdot}1}{39{\cdot}1}\times\ldots\frac{38{\cdot}1+t}{37{\cdot}1+t}\right\},$$

or

$$\frac{Q_t}{Q_0}=\left(\frac{38{\cdot}1+t}{38{\cdot}1}\right)^{-\frac{1}{k}}=\left(1+\frac{t}{38{\cdot}1}\right)^{-\frac{1}{k}}.$$

Consequently, taking the common logarithm,

$$\text{com log}\,Q_t=\text{com log}\,Q_0-\frac{1}{k}\,\text{com log}\left(1+\frac{t}{38{\cdot}1}\right).$$

This is a formula by means of which $\Delta\log P_t$ may be ascertained for any temperature, with considerable precision, when $\Delta\log P$ for any other temperature is previously known.

Among other formulæ it may be useful to mention the following:—

$$\Delta \log P_t = \log \frac{P_{t+1}}{P_t}$$

$$= k\alpha \left(1+\frac{t}{a}\right)^{-\frac{1}{k}} \left\{1-\frac{B_1}{a+t}+\frac{B_2}{(a+t)^2}-\frac{B_3}{(a+t)^2}+\ldots\right\},$$

the values of the coefficients B_1, B_2, &c. being the same as in the original formula for $\log P_t$ expanded in a series. The above series being of great convergency, will often be found useful in supplying defects from using logarithms to seven places of decimals, when logarithms to ten places are not at hand. In the formula for $\log P_t$ one of the factors, viz. $\left\{1-\left(1+\frac{t}{a}\right)^{-n}\right\}$, is sometimes incapable of being correctly expressed beyond the fifth decimal place by means of the ordinary Tables of logarithms to seven places.

Table I.—Elastic Force of Steam of Maximum Density, and rate of Increment per degree, according to Regnault and according to a new formula. (Regnault's Table, p. 624, vol. xxi. *Mém. de l'Académie, &c.*)

Temperature.			Elastic force in hundredths of one atmosphere.		Increment per degree Centigrade.	
Fahrenheit.	Centigrade.	Absolute.	New formula.	Regnault.	New formula.	Regnault.
−22°	−30°	246°	·049	·051	·09509	·09326
− 4	−20	256	·121	·122	·08676	·08360
+14	−10	266	·278	·275	·07943	·07979
32	0	276	·595	·605	·07296	·07359
50	+10	286	1·198	1·206	·06722	·06645
68	20	296	2·286	2·288	·06210	·06179
86	30	306	4·156	4·151	·05753	·05744
104	40	316	7·235	7·224	·05342	·05346
122	50	326	12·11	12·10	·04972	·04980
140	60	336	19·58	19·58	·04638	·04646
158	70	346	30·66	30·67	·04335	·04339
176	80	356	46·65	46·66	·04060	·04060
194	90	366	69·12	69·14	·03809	·03808
212	100	376	100·00	100·00	·03580	·03571
230	110	386	141·53	141·50	·03370	·03373
248	120	396	196·32	196·22	·03177	·03175
266	130	406	267·33	267·15	·03000	·02999
284	140	416	357·88	357·59	·02837	·02836
302	150	426	471·64	471·22	·02685	·02685
320	160	436	612·59	612·05	·02546	·02547
338	170	446	785·04	784·44	·02416	·02418
356	180	456	993·53	992·96	·02296	·02298
374	190	466	1242·9	1242·5	·02184	·02187
392	200	476	1538·2	1538·0	·02080	·02083
410	210	486	1884·5	1884·9	·01983	·01985
428	220	496	2287·3	2288·2	·01892	·01894
446	230	506	2751·8	2753·5	·01807	·01809

TABLE II.—Elastic Force of Steam of Maximum Density, according to new formula, compared with the Elastic Forces indicated by the three elementary Tables from which the adjusted Table of M. Regnault has been formed.

(The force is stated in millimetres of mercury. One atmosphere equal to 760 millimetres = 29·9218 English inches.)

Temperature, Centigrade.	New formula.	Observations engraved on copper, "Graphic Curve," page 608.	Biot formula (H), page 608.	Roche formula (K), page 615.	Regnault adjusted Table, page 624.
° −30	·371	·37	(Not stated.)	(Not stated.)	·386
−20	·920	·91	* ·91	* ·91	·927
−10	2·111	2·08	1·97	2·12	2·093
0	4·520	4·60	4·48	4·60	4·600
+10	9·105	9·16	9·05	9·22	9·165
20	17·37	17·39	17·30	17·62	17·39
30	31·58	31·55	31·50	32·04	31·55
40	54·98	54·91	* 54·91	55·71	54·91
50	92·06	91·98	92·02	93·11	91·98
60	148·82	148·79	148·83	150·19	148·79
70	233·03	233·09	233·11	234·61	233·09
80	354·51	354·64	354·64	356·06	354·64
90	525·33	525·45	525·45	526·42	525·45
100	760·00	760·00	* 760·00	* 760·00	760·00
110	1075·6	1073·7	1075·4	1073·7	1075·4
120	1492·0	1489·0	1491·3	1487·1	1491·3
130	2031·7	2029·0	2030·3	2022·7	2030·3
140	2719·9	2713·0	2717·6	2705·6	2717·6
150	3584·4	3572·0	3581·2	3563·9	3581·2
160	4655·6	4647·0	* 4651·6	4628·5	4651·6
170	5966·2	5960·0	5961·7	5932·8	5961·7
180	7550·8	7545·0	7546·4	7512·9	7546·4
190	9446·0	9428·0	9442·7	9407·1	9442·7
200	11690	11660	11689	11656	11689
210	14322	14308	14325	14302	14325
220	17383	17390	* 17390	* 17390	17390
230	20914	20915	20926	20965	20926

The three formulæ for log P above referred to are

$$b\left\{1-\left(1+\frac{t}{a}\right)^{-1\cdot302585}\right\};\quad a+b\alpha^t+c\beta^t;\quad \text{and } At\times(1+Mt)^{-1}.$$

* Assumed points of coincidence with observations marked on engraved plate of copper.

TABLE III.—Showing, for intervals of 10 degrees Centigrade, the law of Increase according to Temperature, of the Elastic Force of Steam of Maximum Density.

Temperature.		P_t = force in atmospheres.	$\frac{P_t}{P_{t-1}}$ = ratio of increase of force in intervals of 10° C.	$\log \frac{P_t}{P_{t-1}}$, or $\Delta \log P$.	$\frac{\log \frac{P_{t+1}}{P_t}}{\log \frac{P_t}{P_{t-1}}}$.	$\log \Delta \log P$.	$\log \frac{\log \frac{P_{t+1}}{P_t}}{\log \frac{P_t}{P_{t-1}}}$ or $\Delta \log \Delta \log P$.	$\frac{1}{a+t}$ in decimal fractions.	$\frac{1}{a+t}$ in vulgar fractions.
Relative to boiling-point of water (t).	Absolute ($a+t$).								
0°	37°·6	1·0000							
			1·4153	·15085		$\bar{1}$ ·17855			
1·0	38·6	1·4153			$10^{-\frac{1}{38\cdot6}}$		·02591	·02591	$\frac{1}{38\cdot6}$
			1·3871	·14211		·15264			
2·0	39·6	1·9633			$10^{-\frac{1}{39\cdot6}}$		·02526	·02525	$\frac{1}{39\cdot6}$
			1·3617	·13408		·12738			
3·0	40·6	2·6734			$10^{-\frac{1}{40\cdot6}}$		·02463	·02463	$\frac{1}{40\cdot6}$
			1·3387	·12669		·10275			
4·0	41·6	3·5779			$10^{-\frac{1}{41\cdot6}}$		·02405	·02404	$\frac{1}{41\cdot6}$
			1·3179	·11987		·07870			
5·0	42·6	4·7167			$10^{-\frac{1}{42\cdot6}}$		·02348	·02347	$\frac{1}{42\cdot6}$
			1·2989	·11356		·05522			
6·0	43·6	6·1262			$10^{-\frac{1}{43\cdot6}}$		·02293	·02294	$\frac{1}{43\cdot6}$
			1·2815	·10772		·03229			
7·0	44·6	7·8482			$10^{-\frac{1}{44\cdot6}}$		·02244	·02242	$\frac{1}{44\cdot6}$
			1·2656	·10229		·00985			
8·0	45·6	9·9341			$10^{-\frac{1}{45\cdot6}}$		·02193	·02193	$\frac{1}{45\cdot6}$
			1·2510	·09726		$\bar{2}$ ·98792			
9·0	46·6	12·4280			$10^{-\frac{1}{46\cdot6}}$		·02145	·02146	$\frac{1}{46\cdot6}$
			1·2376	·09257		·96647			
10·0	47·6	15·3820			$10^{-\frac{1}{47\cdot6}}$		·02102	·02101	$\frac{1}{47\cdot6}$
			1·2250	·08820		·94544			
11·0	48·6	18·8453			$10^{-\frac{1}{48\cdot6}}$		·02058	·02058	$\frac{1}{48\cdot6}$
			1·2137	·08411		·92487			
12·0	49·6	22·8727			$10^{-\frac{1}{49\cdot6}}$		·02016	·02016	$\frac{1}{49\cdot6}$
			1·2031	·08030		·90471			
13·0	50·6	27·5179							

TABLE IV.—Exhibiting by Logarithms (when the unit of Temperature is 10 degrees Centigrade) the law of Increase of the Elastic Force of Steam of Maximum Density, $\Delta \log \Delta \log P$ being equal to $\frac{1}{a+t}$ nearly.

Absolute temperature $(a+t)$.	log P.	$\Delta \log P$.	$\log \Delta \log P$.	$\Delta \log \Delta \log P$ $\left(=\frac{1}{a+t} \text{ nearly}\right)$.	Absolute temperature $(a+t)$.
24°·6	$\bar{4}$·6886930				
25·6	$\bar{3}$·0831720	·3944790	$\bar{1}$·5960239	·0390873	25°·6
26·6	·4436980	·3605260	·5569366	·0376088	26·6
27·6	·7743170	·3306190	·5193278	·0362599	27·6
28·6	$\bar{2}$·0784530	·3041360	·4830679	·0349725	28·6
29·6	·3590580	·2806050	·4480954	·0337957	29·6
30·6	·6186550	·2595970	·4142997	·0326883	30·6
31·6	·8594300	·2407750	·3816114	·0316624	31·6
32·6	$\bar{1}$·0832758	·2238458	·3499490	·0306902	32·6
33·6	·2918492	·2085734	·3192588	·0297599	33·6
34·6	·4866088	·1947596	·2894989	·0289241	34·6
35·6	·6688199	·1822111	·2605748	·0280911	35·6
36·6	·8396183	·1707984	·2324837	·0273289	36·6
37·6	0·0000000	·1603817	·2051548	·0266011	37·6
38·6	·1508529	·1508529	·1785537	·0259142	38·6
39·6	·2929678	·1421149	·152639	·0252622	39·6
40·6	·4270519	·1340841	·1273773	·0246295	40·6
41·6	·5537435	·1266916	·1027478	·0240477	41·6
42·6	·6736106	·1198671	·0787001	·0234793	42·6
43·6	·7871694	·1135588	·0552208	·0229334	43·6
44·6	·8948872	·1077178	·0322874	·0224372	44·6
45·6	·9971812	·1022940	·0098502	·0219279	45·6
46·6	1·0944385	·0972573	$\bar{2}$·9879223	·0214525	46·6
47·6	·1870084	·0925699	·9664698	·0210253	47·6
48·6	·2752035	·0881951	·9454445	·0205793	48·6
49·6	·3593169	·0841134	·9248652	·0201583	49·6
50·6	·4396153	·0802984	·9047069		

XXVII. *On the Unit-volume of Gases.*

By Professor A. W. WILLIAMSON, *P.C.S., F.R.S., &c.**

FOR many years the term volume has been employed by chemists in a specific sense in relation to the weight of gases and vapours. When a chemist speaks of a volume of hydrogen or nitrogen, &c., he is understood to refer to that particular quantity of the gas which is equal in volume to sixteen parts by weight of oxygen. When we say that alcohol, according to the formula $C^2 H^6 O$, is a two-volume vapour, we thereby say that the weight represented by this formula, namely forty-six parts

* Communicated by the Author.

of alcohol, occupies twice the bulk of sixteen parts of oxygen, or the same bulk as thirty-two parts of oxygen.

Before the rectification of the atomic weight of oxygen, the unit-volume was defined as being the volume of eight parts by weight of oxygen. But retaining the principle of that original definition, viz. that the unit-volume is the volume of one atom of oxygen, we now use volumes double as great as the old volumes, while referring to an atomic weight of oxygen double of what was formerly in use.

A more convenient way of describing the unit-volume now in use is to call it the volume of one part by weight or one atom of hydrogen, and this definition is now generally coming into use. One important advantage of the rapidly prevailing system of applying the term "two volumes" to the volume of two parts by weight of hydrogen (or one molecule of free hydrogen), is that we thus obtain an exceedingly easy means of calculating the density of gases and vapours on the hydrogen scale.

Thus when we say that the molecule of steam occupies two volumes, we thereby assert that H^2O, or eighteen parts by weight of steam, occupy twice as great a volume as one part by weight of hydrogen, and our statement contains the data for calculating the density of steam compared to that of hydrogen; for it amounts to saying that two volumes of steam weigh eighteen, while one volume of hydrogen weighs one; so that one volume of steam weighs nine times as much as one volume of hydrogen.

Again, the molecule of chlorosulphuric acid, SO^2Cl^2, is stated to occupy two volumes in the state of vapour, so that this weight ($S=32$, $O^2=32$, $Cl^2=71$), 135, is as bulky as two parts by weight of hydrogen, and our vapour has accordingly a density of 67·5.

Or when we say that a molecule of marsh-gas occupying two volumes requires four atoms of oxygen, occupying four volumes, for its complete combustion, and that the product is a molecule of carbonic acid occupying two volumes, and two molecules of steam occupying four volumes according to the equation

$$\underset{2\text{ vols.}}{CH^4} + \underset{4\text{ vols.}}{O^4} = \underset{2\text{ vols.}}{CO^2} + \underset{4\text{ vols.}}{(H^2O)^2},$$

we similarly supply the data for calculating the density of each of the aëriform bodies named. Thus CH^4 weighing sixteen, and occupying two volumes, is eight times as heavy as two volumes of hydrogen. O^4 weighing sixty-four is of course sixteen times as heavy as its own bulk of hydrogen. CO^2 weighs forty-four and occupies two volumes, proving that it has a density of 22; and the density of steam is found in like manner.

The great convenience of this hydrogen scale of densities, which is already leading to its general adoption, arises from the fact that it enables the chemist to calculate with facility the density of any gas or vapour from a knowledge of its molecular weight. For inasmuch as every molecule, with very few apparent exceptions, is found to occupy two volumes in the state of vapour, the vapour-density of every compound is equal to half its molecular weight.

For some years past I have been in the habit of using an extension of this natural system of volume-notation, which affords the means of calculating with rapidity the absolute volume of a given weight of a gas or vapour, or inversely the absolute weight of a given volume. This extension consists simply in substituting the word "gramme" for "part by weight" in the definition of volume. A volume of hydrogen is the bulk of one gramme of hydrogen at the normal temperature and pressure, and a volume of any gas or vapour is a bulk of that gas or vapour equal to that of one gramme of hydrogen.

According to the determinations of oxygen, which are less affected by errors of manipulation than the determination of hydrogen, a volume is 11·19 litres at 0° C. and 760 millims. mercurial pressure. For most purposes the volume may be defined as 11·2 litres.

In order to show the advantages derivable from this absolute volume, it will be best to take a few examples of calculations performed by the aid of it.

Thus, it is required to find the volume of oxygen obtainable by the decomposition of one kilogramme of potassic chlorate. The formula $KClO^3$ tells us that the molecule of chlorate weighing 122·5 contains 48 parts of oxygen, so that the proportion $122{\cdot}5 : 48 = 1000 : x$ gives us 391·8, say 392 grammes, as the weight of oxygen contained in our kilogramme of chlorate. In order to reduce this to litres, we have from the density of oxygen (O = 16 = 1 vol.) the proportion $16 : 11{\cdot}2 = 392 : y$, whence $y = 274{\cdot}4$ litres as the measure of oxygen obtainable by the complete decomposition of a kilogramme of chlorate.

2. Given 500 grammes of zinc, required the volume of hydrogen obtainable by its action on sulphuric acid. The equation $Zn + H^2SO^4 = H^2 + ZnSO^4$ tells us that sixty-five parts by weight of zinc displace two parts by weight of hydrogen from its sulphate; and $65 : 2 = 500 : x$ gives us 15·384 grammes as the weight of hydrogen; and this number, multiplied by 11·2, gives us 174·3 as the number of litres of hydrogen.

3. Given 150 grammes of paraffine, find the volume of air required for its combustion, assuming for paraffine the formula

$(CH^2)^n$. According to the formula

$$(CH^2)^n + (O^3)^n = (CO^2)^n + (H^2O)^n,$$

we find that 14 parts by weight of paraffine require 48 of oxygen for their combustion; so that 150 grammes require 514·3 grammes of oxygen, equal to 385·7 litres; $21 : 100 = 385{\cdot}7 : x$ gives us 1830·6 litres of air.

4. Given a room of 80 cubic metres capacity full of air at 15° C. and 760 millims. pressure, what weight of oxygen does it contain? The proportion $105{\cdot}4977 : 100 = 80 : x$ gives us 75·8 cubic metres as the volume of our air reduced to 0° C.; and this multiplied by $\frac{21}{100}$ gives us 15·918 as the volume of the oxygen, whence $15{\cdot}918 : x = 11{\cdot}2 : 16$ gives us 22·740 grammes as the weight required.

5. Required the weight of a litre of ether-vapour, measured at 100° C.

The formula $C^4H^{10}O = 2$ vols., gives us 37 grammes as the weight of 11·2 litres at 0° C., whence 3·305 grammes for one litre at 0° C. 2·42 grammes is therefore the weight at 100° C.

These examples will no doubt suffice to explain the use of this constant, and the advantage derivable from it; and I have found that students of chemistry learn its use very easily, and, aided by it, are enabled to compute rapidly the answers to numerical questions involving a transition between measures of weight and measures of volume.

It appeared most natural to start from weight in fixing an absolute volume, because our symbols are used to denote certain relative weights of elements, and ought to be used as the basis of every calculation.

Equivalent calculations may easily be made by the aid of an absolute volume defined in grains and cubic inches. One grain of hydrogen at the normal temperature and pressure measures nearly 44·5 cubic inches, and 44·5 cubic inches would be the unit-volume of gases for those who use grains and cubic inches instead of the metrical system.

The merest beginner in science understands when he is told that a volume means 11·2 litres, and can easily calculate problems relating to gases, with the aid of this constant. After using the absolute volume for some time, he learns to see the same relations between the volumes of gases in a more general and abstract manner, but the only philosophical beginning for him is an absolute volume defined intelligibly in itself.

XXVIII. *An Account of some Electrical Experiments and Inductions.* *By* J. J. Waterston, *Esq.*

[Concluded from p. 98.]

On the Integral of Electric Force and its mode of distribution in space, as deduced from the elementary laws established by Cavendish *and* Harris.

51. TO prove that *the attraction between parallel electrified conducting surfaces is constant at all distances if the quantity of electricity engaged between them is constant*, suppose two Leyden jars or magic panes exactly the same in every respect, except that the glass of one (L) is twice as thick as that of the other (M). If it takes 100 unit-jars to charge L so that the balance electrometer of Harris connected with its charging side requires 10 grains, there will be required 200 unit-jars to charge M to the same intensity as measured by the electrometer. The attractive force between a square inch of the two coatings of M is four times the attractive force between a square inch of the two coatings of L. Let us suppose the charge of L to be increased by another 100 unit-jars, making the whole charge 200. The force between a square inch of the two coatings of L is now four times what it was with the charge 100. The force is therefore equal to that of a square inch of M, and the quantity of electricity on a square inch is the same in both, while the distance between the coatings is in L twice that in M. The electrometer connected with the charging side of L now requires 40 grains. Suppose the sides of a magic pane to be separated by a dielectric plate capable of compression, and which yields stiffly to the attractive force exerted through it from the opposite sides. *The quantity remaining constant, the static force is constant*, and the intensity of the free part of the charge diminishes as the square of the distance between the opposite surfaces.

52. The coated surfaces of the magic pane may be supposed to be perfect concentric spheres, in which case no free electricity is required to enclose and confine the charge, which is wholly engaged and symmetrically placed. This form given to the magic plane simplifies the subject, and enables us without embarrassment to arrive at an accurate conception of the integral of the force.

Let the sphere A (fig. 25) with conducting surface be situated concentrically within the spherical shell B, also composed of conducting matter. We suppose A to be charged with 100 unit-jars of positive electricity. On the inner surface of B we have negative electricity induced; and the quantity is the same as on A, because they mutually engage; but the density is less, because

the surface is greater, the ratio being the inverse square of the radii.

Suppose 100 radial lines like $a\,b$ to be drawn equidistant, and let c represent an extremely thin disk of metal perpendicular to one of the radii. We know that on the inner side of c negative electricity appears, and on the outer positive. These are shown by the arrow-heads, or rather what are more correctly to be taken as V marks. Thus c towards b presents a positive entrant or male aspect, and c towards a presents a negative recipient or female aspect. Upon each of the 100 radial lines we may imagine an infinite number of such disks without any disturbance of the electricity between the concentric spherical surfaces; and if each is marked like c with the symbol of the electricity that is developed on each side, we shall have each of the 100 lines represented thus >▷▷▷▷▷▷, which exhibits the nature of the static polarizing power that exists in the space between a and b. The intensity of this force depends on the distance between the lines, diminishing as the distance increases. To obtain a clear idea of this, we may suppose another disk d similar to c and close to it. We may also imagine these disks to be extended all round so as to become spherical surfaces C, D concentric with A. The electric equilibrium will not be disturbed. We have next to suppose the electricity or electric lines between A and C to be discharged, also those between D and B. We shall then have remaining the electricity between C and D. Let the radius of A be considered as unity, and that of C be denoted by r; we may then represent cd by dr. Let D be supposed immoveable, and C moveable and expansible. The force that attracts C to D is uniform through the element of space dr, and is proportional directly to the spherical surface C, that is to r^2, the spherical surface of A being unity, and inversely as the square of the density of electricity upon C, that is to $\frac{1}{r^4}$, the density upon A being considered unity. Hence the static force of C towards D is thus $\frac{r^2}{r^4} = \frac{1}{r}$ [supposing the static force of A, when as originally charged towards an exterior concentric spherical surface at distance dr (the electricity being supposed engaged between the two as in the case of C and D), to be unity], and the space through which this static force $\frac{1}{r^2}$ acts being dr, the differential of the work performed by the surface C in expanding to D by the influence of the electric force is $\frac{dr}{r^2}$, the integral of which is $1 - \frac{1}{r}$. This gives us the value of the work performed by the spherical surface A expanding to infinity under the influence of the

electricity with which it is charged. This value is expressed by unity, which means the static force at A acting through the radius of A.

53. Compare the charge on A with a charge of equal density on another sphere Z with twice the radius of A. The quantity of electricity on Z must be four times that on A to have equal density with it; and the density being equal, but the surface of Z four times that of A, the static force on Z is four times the static force on A; and as the integral is that force acting through the radius of the respective spheres, it comes to pass that the integral of the charge on Z is eight times the integral of the charge on A; and the quantity of electricity on a square inch of the surface of Z, although equal to the quantity on a square inch of A, has twice its mechanical equivalent.

This may seem somewhat of a paradox; but a little consideration as to the rate of divergence of the electric lines will make it clear. Thus let $c\,d$ (fig. 26) represent the space occupied by an electric line or root on the surface of the sphere A, and $e\,f$ the same on the surface of Z, b being their common centre. Now, the density being the same, $cd=ef=\alpha$, and ϕ is the static force common to both. These lines on proceeding outwards diverge, so that if cd or α becomes $\alpha\rho$ in δ distance out from the surface of A, ef or α becomes $\alpha\rho$ in 2δ distance out from surface of Z. The statice force of both being ϕ, we have $\phi\delta$ the first dynamic increment of a line issuing from surface of A, and $2\phi\delta$ the first dynamic increment of a line issuing from the surface of Z. All the successive increments have the same ratio, therefore the integrals have also this ratio.

54. Thus we see by clear induction from the elementary laws established by the experiments of Cavendish and Harris, that an electrically excited surface contains only the roots of lines of force, in which lines the mechanical power of the electricity resides. Work is, as it were, stored up in these lines, and the static intensity at a point in one of them depends on the closeness of the neighbouring lines at that point only. The closer they are packed the more intense the longitudinal force, and the more intense the lateral force of repulsion. Indeed these forces, as they increase and diminish together, seem to be identical, or a mode of reaction. The repulsive tension exists in the plane $m\,n$ (fig. 27) transverse to the electric line $l\,p\,l$ in which the contractile tension is manifested.

When two bodies charged with the same electricity are brought closer together, the lines become more closely packed. Mechanical power, or work, requires to be expended in order to force the bodies closer together, *i. e.* to force the electric lines closer together. The lateral repulsion of the lines has to be overcome,

and that increases in a high inverse ratio of their mutual distance, and produces a proportionally intense longitudinal contractile static tension. The mutual distance varies at every point of a line, and responsively so does the transverse repulsive tension and its dependent longitudinal contractile tension.

Let the disk *c* (fig. 25) have a magnified representation in fig. 28, and let it be composed of a double film of metal $c_1\, c_2$, the two films being perfectly close together, yet not cohering. We may further imagine these double films to extend all round to form concentric spherical surfaces as before (52). It will be remarked that we have negative or $\underline{\vee}$ roots developed on c_1, and positive or $\triangle$ roots on c_2, the lateral repulsive tension acting in the space between these roots on both sides of the duplex film *c*; but on the side c_1 it produces a longitudinal strain in one direction, *i. e.* towards *a*; and on the side c_2 it produces an equal longitudinal strain in the reverse direction, *i. e.* towards *d* or *b*.

Let *a*, *x*, *b*, *z* (fig. 29) represent four rods jointed at their extremities; press *a* and *b* towards *c*, this will press *x* and *z* from *c* in opposite directions. A rough notion may thus be obtained of the lateral and longitudinal strain that exists on every part of an electric line—the lateral convergent being the *a b* pressure, and the longitudinal divellent being the *x z*.

The lateral force affects the position of the lines, and thence of their polarized roots on conducting surfaces; but it is the longitudinal strain at the polarized roots that immediately produces the phenomena of motion and discharge. The motor phenomena resulting from the strain at the roots sometimes assume the appearance of attraction and sometimes of repulsion in adjacent bodies, as, from the lateral action of the lines, the roots happen to be distributed more on their near or their opposite sides.

55. If we examine closer this quantitative relation between the lateral and longitudinal forces, we shall find a certain simplicity that is worth keeping in remembrance.

Suppose two planes at the infinitesimal distance *dr* to cut the lines at right angles. The lines intersect these planes in points, each point, as *p*, fig. 30, being the centre of a certain extent of surface which may be denoted by α, and *dr* may be viewed as the axis of a cylinder whose area is αdr. This area is small when the lines are closely packed, and *vice versâ*. What relation does the static intensity of the longitudinal force at *p* bear to the area of this infinitesimal cylinder?

The force, as it has been proved from Harris (51), is as the square of the density of electricity, or as the square of the number of points in a square inch. Thus α being the area of one

point, the number in a square inch is $\propto \frac{1}{\alpha}$, and the force on a square inch is as $\frac{1}{\alpha^2}$; but the number being $\propto \frac{1}{\alpha}$, the force of each one is $\propto \frac{1}{\alpha}$.

Thus it appears that the fundamental law of action is that the static longitudinal pull at p is inversely as the area of the infinitesimal cylinder αdr. Now to fix this in the mind, we may imagine the cylinder to be a reality, and, however great or however small, to contain the same constant quantity of an elastic fluid, which will thus be more compressed when α is small than when it is large. Further, let us suppose a valve or pressure-gauge at p, or rather two small pistons, acting one in the direction pl_1, and the other in the direction pl_2, under the influence of the elastic fluid contained in the cylinder. This elastic fluid, when of double density, exerts double pressure, according to the law of Mariotte; so that it is not difficult, by keeping this little artificial arrangement in view, to retain a distinct idea of the distribution of force in an electric line.

How Nature accomplishes this curious arrangement of force, and supports it from two excited material terminals, is of course a profound mystery; but that it does exist is, I submit, a fact inductively established. We have no choice, we cannot deviate from it, if we regard accuracy in the conception of the potential nature of electricity. It is a theory, not an hypothesis; a system proved from Harris's observations, not a system coined in the imagination.

56. With regard to practically determining the mechanical equivalent of a given charge (that is, a given quantity of electricity on a given surface), the only difficulty is from the portion that is free—how to ascertain its amount and allow for it.

The following occurs as perhaps practicable with Harris's apparatus. [The unit-jar as a measure of *quantity* seems to be unexceptionable; would it not be well to have an arrangement for an *absolute*-unit standard measure?]

Suppose we wish to compute the integral of a charge of g unit-jars given to a sphere 10 inches in diameter, having a surface of 314 square inches. Let B be a circular conducting plate of 100 square inches mounted horizontally in a perfectly insulated condition, and at the distance of an inch let A, another circular plate of the same size, be suspended by newly high-dried silk lines to one side of a balance. Let a charge be given to B by unit-jar, while a free conductor touches A; and after removing the free conductor from A, let the weight required to raise A be measured as in Harris's experiments. Next let B be touched with a free conduc-

tor, and, the distance A B continuing to be 1 inch, let the weight required to raise A be again measured. Let the first weight be denoted by m, and the second by n, and let the charge given to B at first be c unit-jars. The forces at the constant distance A B being as the square of the quantities of *engaged* electricity between the opposite surfaces, we have $\sqrt{m} : \sqrt{n} :: 1 : \sqrt{\frac{n}{m}}$; and $c\sqrt{\frac{n}{m}}$ is the charge which, engaged between two surfaces of 100 square inches at the distance of 1 inch, engenders the force m of apparent attraction. Now $c\sqrt{\frac{n}{m}}$ upon 100 square inches gives electricity of same density as $3{\cdot}14\,c\sqrt{\frac{n}{m}}$ upon 314 square inches, the surface of the sphere of 10 inches diameter: compare this charge with g. The force of apparent attraction follows the ratio of the square of the charge; hence we have

$$c\sqrt{\frac{n}{m}} \times 3{\cdot}14 : g :: 3{\cdot}14 \times m : \frac{mg}{c}\sqrt{\frac{m}{n}} = w,$$

that is, the value of the weight that, acting through 5 inches, the radius of the sphere, expresses the integral of its electric charge —its work-representative.

On the Arrangement of Electric Lines into Systems.

57. From the simplest system, viz. that of lines issuing from the surface of an insulated conducting sphere, we may pass to others where exact mathematical treatment seems hardly possible as yet. But one or two salient points arrest the attention.

58. Free electricity in a conducting surface of unequal curvature, as the solid S B with a sharp and blunt end (fig. 31).

a. The mechanical equivalent of a line upon such a surface increases in going from the blunt to the sharp end. The lateral force of the roots lying in the small circle *a*, resolved perpendicular to that circle, must equilibriate the lateral force of those in the larger circle *b* similarly resolved. This requires closer packing at *a* than at *b*. The number in $a \times$ their lateral force should be equal to the number in $b \times$ their lateral force. Let a be the length of one circle, and b that of the other; m the number in *a*, and n the number in *b*. Then $\left(\frac{a}{m}\right)^2$ and $\left(\frac{b}{n}\right)^2$ represents the area of each root respectively; and since the lateral force at the root is inversely as the area, we have $\left(\frac{m}{a}\right)^2$ and $\left(\frac{n}{b}\right)^2$ representing

the lateral force at each root respectively in the circles a and b. Multiplying these by the number of roots m and n, we have $\frac{m^3}{a^2} = \frac{n^3}{b^2}$. Hence $\frac{m^3}{n^3} = \frac{a^2}{b^2}$; and the density at a being $\frac{m^2}{a^2}$, and at b $\frac{n^2}{b^2}$, we have the ratio of the density at b to that at a as 1 to $\frac{b^2}{a^2}\frac{m^2}{n^2}$, which by substitution is equal to $\left(\frac{b^2}{a^2}\right)^{\frac{1}{3}}$.

b. Suppose a continuous metallic envelope to intersect all the lines that radiate from the surface of S B beginning at a certain point P. If P were at an infinite distance from S B, it is evident that the envelope would be spherical: so at intermediate distances such envelopes would graduate between the extremes of such a surface as S B and a perfect sphere, while those close to the primary S B would be less contrasted than it in the curvature of its extremities. The surface would be similar to S B, but less and less pointed as the distance is greater.

c. The mechanical action of the electric roots on S B being at each perpendicular to the surface, the integral of all these forces, resolved in one direction, ought to be zero, because all the lines being supposed free, and no discharge taking place at the sharper end, the body is at rest; the static pull at the surface is equal in all directions.

59. Free electricity of the same kind on two adjacent insulated conductors.

a. While the excited bodies are forced together, the lines are closer packed and have their integral power augmented, mechanical power being converted into electric tension, and *vice versâ*. When they fly from each other, electric tension is converted into mechanical force or work, *e. g.* the two hemispheres H, K (fig. 32); when separate and equally electrified, their collective surface measures six great circles, when in close contact it measures only four.

b. The repulsion between H and K is immediately caused by the increased quantity of electricity accumulated on the off sides of the hemispheres (leading to a preponderance of *root pullings* on these sides), which is effected by the lateral repulsion of the lines that issue from H on those that issue from K bending them round.

c. The disposition of the electricity on the adjacent sides of the hemispheres as they approximate must be peculiar. The lines belonging to one that lie next to the lines belonging to the other, as *ab*, *cd* (fig. 32), while approximating at *a* and *c* and external to these points lying adjacent, and forming part as it were of one system, must at the lower part *b* and *d* diverge to these

roots on different surfaces, there to form the interior terminals enclosing a space in which no electric lines exist. I have found this confirmed by experiments made with circular plates 5 inches in diameter stuck on cylinders of lac (fig. 34). Narrow slips of gold-leaf about $1\frac{1}{2}$ inch long were stuck on at three or four points in a line passing through the centre, so that when the plate was charged they rose on end, and would stand thus for an hour (d, q). When the twin-plate p, equally charged, was brought down parallel to q, there was a certain distance (about 3 inches) at which they dropped suddenly dead as it were; and looking through a lens at them when the distance was reduced to about 2 inches, there was not the slightest appearance of electric excitement at the edges of the leaves when p was tilted a little so as to disturb the general equilibrium of the system of lines.

It appears that in such cases, when bodies similarly electrified are made to approach, we are compelled to admit the existence of a series of *inner terminal lines* acting laterally outwards and sideways, and sustained by their contractile tension. (See 60.)

60. When insulated conductors charged with different electricities approach each other, the charge becomes more engaged and less free. The mechanical integrals of the lines diminish, and electric tension is converted into work. When forced separate it is *vice versâ*.

Let Q and R (fig. 36) represent two equal spheres segmented at q and r so that there should be equal and parallel circular planes opposing each other at those points. Suppose these spheres to be equally charged with the opposite electricities, and to move backwards and forwards in the line Q R—an impervious dielectric medium, such as a mica plate, intervening between the circles q, r to prevent discharge when close together.

When Q and R are at a distance, almost all the electricity upon them is free and distributed over their spherical surface (fig. 36); when they are close together, almost all the electricity is engaged (fig. 37), and to be found in the opposite circles q, r separated by the thin impervious non-conducting film. While approaching, they are impelled by the prepondering force of the electricities (or *root pulling*) on the near sides, and work or dynamical force is engendered. The electric lines collapse, and their integral force is expended by being converted into motion. They are to a certain degree discharged; but complete discharge has not taken place, for the roots are still polarized, and by applying force the spheres may be withdrawn from each other and work reconverted into electric lines.

Let us consider the equilibrium of the lines in an intermediate position such as fig. 35: what is the force that packs close together the lines on the near surfaces? The condition of equi-

librium among the lines requires that they be pressed as much inwards towards $q\,r$ as outwards; but tracing round the spherical surface till we come to the extreme outward points opposite q and r, it is obvious that, although the excited roots of these points may have others adjacent mutually repellent on all sides, yet the lines that issue from these extreme points must in their curvilinear course $l\,m$, $u\,v$ take directions that cannot have juxtaposition except at the issuing points on the surface. What, then, prevents them yielding to the repellent forces of the interior adjacent lines? Let m (fig. 38) represent an ultimate metallic chemical atom in an electric line P N. If such a condition could be realized, there is every reason to believe that the side towards N would be positively electrified, and the side towards P negatively electrified, as represented by the V symbols, and that it is thus being pulled equally from both sides with intensity in accordance with the nearness of adjacent lines. Now let us suppose a similar atom situated at each symbol thus (fig. 39). We have, instead of an electric line, a line of electrified molecules each of which is drawn by an equal force in opposite directions, giving to the whole line a *longitudinal contractile tension*. Now supposing such a line to be curved, this contractile force will engender a lateral compressing power towards the concave side; and as all the disengaged electricity in this system consists of lines similarly curved, *i. e.* with similar convexity one over the other, *the lateral packing power is cumulative*, and the central engaged lines will be packed together by means of the curvilinearly derived power of the whole of the system exterior.

The proof of the existence of this contractile power in a line is obtained by employing the method of concentric films as in 52. Suppose B and all the concentric films to contract simultaneously and concentrically upon A by the electric action between the adjacent surfaces in degrees respectively proportionate to their distance from it. Here is a system of lines expending their integral, and in doing so exerting a contractile force throughout, which must therefore have had a contractile tension as potential antecedent, a force acting at each point of a line towards its opposite extremities. A line composed of india-rubber has a contractile force which might be thus defined; but it differs from an electric line in this, that in the rubber line the force is the same at every point; but in the electric line the force depends on the distance of adjacent lines, and is thus variously distributed according to circumstances. Thus, *e. g.*, in the concentric spherical arrangement of 52. the contractile tension diminishes outwards regularly. When the electric lines are closely engaged, the contractile tension must be nearly uniform. In the systems figs. 34, 35 it may be small at the root, gradually

increase for a certain distance, obtain a maximum, and then diminish. Generally it must be in a continual state of change according to the varying condition of the system to which it belongs.

61. In the magic pane the free part of the charge compresses the engaged part by the contractile tension of the free lines. These by their lateral repulsion tend to fill up unoccupied space, and thus bend round the engaged lines and acquire the lateral packing power by their longitudinal tensile force when thus bent.

Fig. 40 is intended to give an idea of the charging of a magic pane. The line B pressed forward by A (intended to represent the lines engendered by the machine) arrives at the balls k, l. The ball l cuts the line, and so do the opposite surfaces of the magic pane. The part of it between k and l remains, and so does the part between the coatings of the magic pane. The part c between l and the near surface of the magic pane contracts and discharges itself along the conducting wire w; the exterior part D escapes to the ground by the free conductor x after the centre part N has been pressed into the magic pane. The lines between k and l accumulate until discharge takes place, which may be looked upon as a signal that a certain number α of lines *have* crossed over and left segments packed into the magic pane. The spark appears *after* the unit quantity has entered the jar. The propelling force (from the machine) required to bring up α of course increases as the charge M increases; but when brought up, it is probable that discharge between k, l destroys the same constant number of lines, and thus that the unit-jar correctly measures quantity, its coatings i, j being viewed as prolongations of the surfaces of the respective balls.

Remarks.

In experiments such as 1, 3, 5, 6, the phenomena are, strictly speaking, examples of convection or conveying of polarized particles (except 5*b*, 5*c*) to non-conducting matter, upon which they adhere without being depolarized. They all tend to prove that electricity is never separate from matter even in discharge.

(3.) It seems probable (since a small ball throws off stars) that a brush consists of a symmetrical succession of polarized molecules—at least when discharged upon a non-conducting surface, upon which they cling still in an excited condition, and therefore must act repellent upon the next in succession (see 4). The breadth of the fosse is evidently caused by capillarity.

(7.) In this experiment the thin shell-lac coating of the conductor becomes charged as magic pane with one side only coated. Viewed in the dark, the brush was of eccentric forms.

(8 *a*.) Is a remarkable effect that I do not recollect having seen

mentioned before, although it can hardly have escaped the notice of experimenters.

(8 *b*, &c.) Are due to a similar quality of shell lac, and probably other non-conducting surfaces. If the electric fire consists of strongly excited molecules, we may remark that when in such condition of excitement, and moving probably with inconceivable velocity, they appear not to obey any ordinary law of projectile force. They seem for the time to form part of the polarized æther, and to be in the grasp of an agent of transcendant attributes. As it is in the spark, so must it be in the lightning, the amazing mechanical effects of which must be due to the matter issuing from the surfaces of discharge.

(9.) Such phenomena might be anticipated of a heterogeneous body consisting of a *perfect* dielectric substance (if such a substance really exists, but clear amber is not) containing conducting particles equally distributed throughout its mass, each one particle being separate and isolated from its neighbour. The apparent induction at first seems evanescent; but in subsequent experiments it will be remarked that, if exposure lasts for some minutes, persistent inductive excitement appears on the surface adjacent to the one excited.

(10.) We may remark in this, that while brush discharge and convective discharge of less intensity (1) takes place upon a non-conducting surface, the spark discharge does not take place without forcing it open: the coating is *lifted up* at both discharging surfaces, *therefore metallic particles issue from both.* Is such the case in lightning between the cloud and the ground? Does the flash consist of luminous water on one side, that of the cloud? Towards the ground there may be many surfaces of discharge, at each of which polarized molecules issue giving rise to so many different foci of explosion.

(21.) This effect is what would take place by separation of polarized molecules from the metal that strokes the wax surface, because the excitement is located on the surface stroked. Yet it is difficult to see that this is likely, the wax being much softer than the metal, especially when it is steel—as the side of a steel blade.

(22.) The rate of discharge diminishes with the intensity: combustion being chemical action, and chemical action being also electric action, the electrically-excited molecules of the oxygen, carbon, or hydrogen may fly to the excited body under the influence of its induction, which though weak may yet be stronger than that of the combining element. From 29 *d*. it appears that molecules do actually proceed from the flame to the near surface of the wax screen, which, with the B. D. and intermediate air as dielectric, becomes a charged magic pane arrangement, the B. D. forming one side and the screen the other.

(24, 25, 26.) Here there is evidence, especially in 25., that the flame or match acts in discharging the B. D. partially, as if unconnected with the electroscope. In this it differs from the action of points, which could not discharge the B. D. without charging the electroscope, although the discharge would of course be more limited than if the plate of the electroscope were a free conductor.

(27, 28.) Why should not the brass disk be discharged under the same conditions that discharge the excited sulphur?

(30.) This is proof of induction acting in curved lines, which Faraday was the first to direct attention to (Experimental Researches, 1218, 1231). Not entertaining the possibility of such curved action taking place in a vacuum, he holds that the inductive force is enabled to bend round in consequence of the molecules of air being polarized, and one leading the action to the others in straight lines. In § 63 of paper in Phil. Trans. for 1834, Harris states that the operation of electricity on distant bodies by induction is quite independent of atmospheric pressure, and is precisely the same *in vacuo* as in air.

(31.) The free part of the charge which issues from the exterior surface of the inside coating gradually diminishes, and the engaged or masked portion between the coatings consequently expands, and the free part comes to issue from the exterior surface of both coatings. So the electroscope shows latterly the action of that nearest to it, which again is removed by touching the outer coating. See 34.

(35.) The diminution of the excitement probably takes place by discharge (the exact nature of which remains to be discovered). This must take place, not only on the excited side, but all over. Now remark that the discharge of the electric lines that takes place on the excited side is the total of each line discharged; but the discharge that takes place on the side opposite the excited side is the discharge of lines that pass through the cake, so it must leave undischarged that part of each line that lies between the two surfaces, having negative root on α and positive on β. A charged magic pane without coatings is thus gradually formed and gradually also discharged. It is the engaged lines becoming gradually disengaged that at last brings out the positive electricity on the surface opposite to that originally negatively excited.

(37.) The inductive effect on a wax surface is very marked in this and other experiments. If a B. D. charged negative is placed resting on a cake of sulphur, it induces gradually a persistent positive on that part of the surface of the sulphur upon which it rests. Time is required. See 39, 40.

(38.) When first discharged this double cake has positive on

the whole of the outside surface, top, bottom, and sides, which is completely engaged by the negative on the two interior surfaces. No free charge is required to hold the engaged lines together; the condition is similar to the concentric spheres of 52.

(41, 42.) The appearance of positive on β depends on a partial discharge having spontaneously taken place during the elapsed time, as in 35.

(43.) The lower cake positive by induction; the upper by partial discharge of a few of the negative lines that pass upwards through the three cakes. It is remarkable that the inductive power is limited in its action to the surface in contact with the surface excited.

(44.) In this, as in 36., the surface in contact with the excited surface appears to share, to a small extent, its excitement, as if conduction had actually followed very slowly behind induction.

(45.) The permanent effect on the cylinder was similar to the temporary effect on a conducting cylinder of the same size, the charged B. D. being supposed close to but not touching it.

(47.) This is a very instructive experiment; there was no actual loss of charge, only an apparent loss, so long as the cake was on the electroscope and the contact surface inductively excited.

(53.) The large radius of the hoop appendage that characterizes Winter's electric machine gives slow divergence to the electric lines that issue from it. See 53. This gives them, as part of the system of electrical lines that includes the lines between the spark balls, great power of lateral compression upon them previous to the spark.

The thunder-cloud as a charged surface is an extreme example of the spark-producing power of slowly converging electric lines.

(58 *a*. and 58 *c*.) The distribution computed from 58 *a*. subjected to the equation in 58 *c*., ought to stand the test. In an ellipsoid or spheroid it might be practicable to execute the calculation, and thus obtain further confirmation of the law of mutual dependence of the lateral repulsive and root-pulling or contractile force.

In conclusion I may mention that the theory of electric lines here given was deduced from Harris's experiments about twelve years ago; since which time I have been in the habit of applying it to the published results of experimentalists, and thus continually testing it. It is very suggestive of new experiments. Some of the simpler sort of these I have been able to make, but there are others, chiefly with respect to the production of light and mechanical effect, that require greater means and appliances, not to mention aptitude; for to suggest and to execute are spe-

cialities that do not always go together. The main end and purpose of these would be to obtain some idea as to the working arrangements between the æther—that higher potential form of matter in which the might of the Infinite resides—and ordinary molecules, the agents of its development. If we confine our attention to the planetary movements, nothing seems clearer than that its density must be inappreciable. On the other hand, were we to make legitimate inferences from the most obvious phenomena of radiant heat, there is evidence that its density may not differ much from that of water, and at least that it is quite impossible that its non-resistance to the celestial motions can be owing to its extreme rarity.

Edinburgh, December 4, 1864.

XXIX. *On the Conservation of Force.* *By* Dr. C. K. Akin*.

ABSENCE, and another controversy of a very different nature, prevented my noticing hitherto Professor Tait's answer to my remarks published in the last December Number of this Magazine. Professor Tait begins by calling attention to the fact that, although omitting the words "in omni instrumentorum usu" from the passage which he quoted from Newton's *scholium*, he indicated the omission by dots. The readers of this Magazine will have seen that in reproducing from Professor Tait's paper the paragraph in question, I took care to cause the dots also to be inserted, to which I made special reference in my remarks. On this point, therefore, there can be no misunderstanding. But when Professor Tait says that "in ordinary mechanics" is the "perfectly complete" free rendering of the above Latin words, I can only partially agree with him. No doubt the rendering is free, "not literal," and in some instances it might also be correct; but I contend that in the present case it is not properly admissible. In the sentences preceding the one cited both by Professor Tait and myself, Newton instances expressly the cases of the "balance," "pulley," "clocks," "screw," and "wedge"; and in my opinion, therefore, the free English translation of "in omni instrumentorum usu," as applicable to the case in hand, is not "in ordinary mechanics," but as given by Motte, "in the use of all sorts of machines," or something like it.

Professor Tait allows that "in Newton's time, and long afterwards, it was supposed that work was *absolutely lost* by friction" —in other words, that Newton himself supposed it to be so; but, considering that it was known that friction excites heat, as well

* Communicated by the Author.

as the other facts I have mentioned, I cannot agree with Professor Tait "that, so far as experimental facts were known in Newton's time, he had the Conservation of Energy complete." In the case of any other man it might appear ungenerous to look too closely into claims to a scientific discovery put forward on his behalf by well-meaning advocates, especially when there is not any better-entitled competitor in the field; but in the case of Newton, whose head is already so thickly covered with laurels, this remark could not apply. I cannot help thinking that the principle of the Conservation of Force, in its widest sense, was discovered by no single person, but was only gradually evolved and developed; and I am mistaken if we are already in full possession of its meaning.

Professor Tait protests against the allowing of any weight to the opinion of John Bernoulli "on a question of this nature," because he "seriously demonstrated the possibility of a perpetual motion." I am not aware that, in point of theory, "perpetual motion" is impossible; although, no doubt, "perpetual work" is. But waiving altogether this point, would it not be possible also on such a principle to impugn the value of any opinion of Newton—for instance, on the subject of light, on the plea that his fundamental notion of the nature of light was wrong?

Like many others, I am anxious for the appearance of Professors Tait and Thomson's long-promised treatise; and in the meanwhile the "Sketch of Elementary Dynamics," published for the use of the students of Glasgow and Edinburgh, might perhaps with advantage be made more accessible to students in general than I have understood it to be.

London, February 1865.

XXX. *On the Origin of the Alpine Lakes and Valleys. A Letter addressed to* Sir Roderick I. Murchison, *K.C.B., by* M. Alphonse Favre, *Professor of Geology in the Academy of Geneva, and Author of the Geological Map of Savoy**.

Sir, Geneva, 12th January, 1865.

I AM glad that you have asked my opinion of the new theory, according to which the Alpine lakes have been excavated or scooped out by glaciers; and of that which also explains the origin of the Alpine valleys by means of the erosion produced by glacial action†.

* Communicated by Sir Roderick I. Murchison, K.C.B., D.C.L., F.R.S., &c.

† A great many arguments against these theories have been advanced in various memoirs, as in those of Mr. Ball (Phil. Mag. 1863, vol. xxv. p. 81), Desor (*Revue Suisse*, 1860), Studer (*Archives des Sc. Phys. et Natur.* 1863, vol. xix.), &c. However unwilling I may be to reproduce the arguments which they have already employed, it is almost impossible not to revert to them occasionally.

I am a strong partisan of the notion of the transport of erratic blocks by ice, at the period of the great extension of the glaciers, and as a Swiss I am attached to this theory, which is worthy of the term national. But, at the same time that I acknowledge it to be accompanied by certain difficulties, I cannot comprehend the two other theories, although they have the advantage of being advocated by able men of science. Amongst these is to be counted Professor Ramsay, a highly distinguished geologist, to whom long practice on the Geological Survey of England has given great powers of observation and a sure eye (*coup d'œil*), Mons. de Mortillet, who is well acquainted with the Alps, and Professor Tyndall, whose works on physics hold the first place. Not that I do not sincerely respect the opinions of the learned authors who have developed these views, and who have done so, I acknowledge, with considerable ability.

It is evident, indeed, that existing glaciers abrade the rocks on which they move, inasmuch as they polish them. But this action is so feeble, that I cannot see how it has been inferred therefrom that it has been able to scoop out deep lake-basins many hundreds of feet below the mean level of the valleys, even on the supposition that it has been exerted during very long periods. I understand still less how this same action could have excavated valleys many thousands of feet deep in a great rock-mass like that of the Alps.

A limit must be set to certain effects. This limit exists in all geological questions, and it is indispensable to establish it.

On seeing a dune on the sea-shore, twenty or thirty metres high, formed by means of grains of sand driven by the wind, shall I be right in concluding that in some hundreds of thousands of years this same dune could attain the height of the Alps or that of the Himalaya?

I have no wish to maintain that the glaciers have not exerted any influence on the forms of lakes and valleys. It seems to me to be impossible that masses so considerable as those which moved in the valleys during the glacial epoch, should not have fashioned, more or less, the *borders* of these depressions. But I cannot become an advocate of the belief that glaciers are the original cause of the formation of lake-basins and valleys. I believe both to be a direct consequence of the formation of mountains, and that they both owe their origin to movements of the earth's crust.

Let us now leave these general arguments, and arrive at more precise facts relative to the origin of the Lake of Geneva. According to all glacial theories, the union of all the glaciers of the Valais at Martigny, to a portion of those of the main body (*massif*) of Mont Blanc, formed one enormous glacier, to which

the name of the Glacier of the Rhone has been given. This glacier evidently discharged itself into the Swiss plain by the valley which extends from Martigny to Villeneuve, and had a minimum thickness of 2300 to 2600 feet *.

This great glacier extended itself over the plain. It covered all the bottom of the basin of Lake Leman with moraines, boulders, clay, and scratched pebbles. The distribution of these materials has often been studied; they are spread over the two banks of the lake; but I do not think that any conclusion can be drawn from their examination, either for or against the hypothesis which I am desirous of examining.

In the course of its slow but continuous movement, the enormous glacier abutted eventually against the Jura. As M. Charpentier has stated, it is remarkable that the maximum height of the traces which it has left should be near Chasseron, a mountain situated to the north-west of Yverdun, just opposite the valley of the Rhone. The blocks there attain an elevation of 3000 feet above the Lake of Neuchâtel †.

Thence the upper limit of the boulders falls successively towards the north and south, in such a manner that we may apply the term *median* to the line which connects the mouth of the Rhone near Villeneuve with Chasseron. North of the median line the higher line of the blocks rejoins the plain in the environs of Soleure. The glacier terminates there, and has left at its extreme limit the remarkable blocks of Steinhof, near Soleure. South of the median line the glacier has left incontestable traces over all the southern extremity of the Swiss plain. It has gone beyond the limit of this plain in passing Mont Sion, south of Geneva, and the defile of Fort de l'Ecluse. These facts have long been known; but it is a matter of surprise (reasoning according to the hypothesis of the excavation of the basin of the lake by the glacier) that the lake has not been hollowed out in the direction of the median line—that is to say, from the mouth of the Rhone to Chasseron—but in a curve which bears no relation to that line.

This bend in the lake nearly follows the base of the great mountains which are situated on the southern bank, at least so far as the large lake is concerned. The depth of the basin is evidently connected with the neighbourhood of the mountains, and the inclination of the strata; it is thus that near Meillerie, where the mountains are elevated and the strata vertical, the lake at-

* Charpentier, 'Essai sur les Glaciers,' pp. 270, 271. I am led to believe that the glacier rose above this limit, and that if blocks are not met with above it, it is owing to their having rolled towards the bottom.

† Charpentier, ibid.

tains its maximum depth (265 metres near Meillerie, and 300 metres a little further west*).

Nevertheless it is probable that in this locality the bed of the lake is of the same nature as its banks, that is to say, limestone, and that the ridges there consist of very hard limestone. Further westward, where the lake is situated in Tertiary Mollasse much softer than the limestone, it only attains a depth of from 30 to 40 metres.

This fact is of considerable importance. It seems to me inexplicable, on the supposition that the glacier hollowed out the basin of the lake; on the other hand, it is easy of explanation in connecting the depression of the lake with the inclination of the strata. Near Meillerie the beds of hard limestone are vertical and highly contorted, and there the lake is deep; nearer Geneva the softer beds of Mollasse descend from the two sides of the lake beneath its waters with a gentle inclination, and there the lake is shallower. This proves the relation between the depth of the lake and the flattening of the beds, as has been already stated by M. Studer; and I will show further on, that it is connected with the reversal of the same beds.

Let us now turn to another point.

I consider that the observations made in the neighbourhood of Geneva have contributed to the origin of that theory of the erosion of the lakes, which I oppose. These observations may be summed up as follows.

Below the Lake of Geneva there are found considerable accumulations formed in their upper part of glacial deposits (clay with scratched pebbles and transported boulders), and in their lower part of the older drift of Necker. This latter deposit is different from the old drift of the greater number of the savants who have written upon the geology of France. We have also in our country this old drift, which is that which I have distinguished in the explanation of my geological map of Savoy by the name of terrace-gravel (*alluvion des terraces*). It contains *Elephas primigenius*. Overlying the glacial drift, this last is higher than the older drift of Necker, upon which I am desirous of making a few remarks. This last is composed of rolled pebbles and sand, often bound together by a calcareous cement. No striated pebbles are seen amongst them. The principal characters of this accumulation in our district consist in its being of older date than the glacial deposit, in being placed below the Lake of Geneva, and in enclosing pebbles (such as those of euphotide) which could not have been derived from the Valais, whence the Rhone glacier proceeded.

* Chart of the principal soundings of Lake Leman, by H. T. De la Beche, 1827.

These pebbles must consequently have passed over the depressions of the lake. But how could they do so, since their transport appears to be anterior to the development of the glacier? That is the difficulty, and it is this which has given rise to the notion of the theory of excavation, in which it is supposed that the pebbles of the older drift have been heaped up by pre-glacial currents in the depths of the lake, and that when the glacier reached them it excavated that portion of the lake which had been filled up. It is supposed, then, that it has produced a great excavation, and that it then spread before it all this enormous mass of pebbles which it drew from this great depression. This idea, generalized and applied to other localities, has produced the hypothesis which is known by the name of the theory of excavation. I think I have been impartial in this explanation.

To this theory I believe I am able to offer objections which seem to me to be very serious. In the first place, when the glacier originally began to carry away from the depths of the Lake of Geneva all the enormous mass of pebbles which is now deposited lower down, how did it effect it? Did the glacier slide over the solid rock without leaving any intervening mass of these pebbles between the two? For if it left beneath it no pebbles, it ought to push before it an enormous mass of this débris, such a mass as can with difficulty be represented—a mode of action which would be the more singular, because nothing amongst existing causes countenances this supposition, no part of the glacier being seen to push before it an accumulation of *rolled* pebbles. If, on the contrary, the glacier covered these rolled pebbles again, the excavation seems to me to be very difficult, because the glacier moulds itself upon its under surface, and causes the pebbles of the underlying bed of mud to advance very slightly.

Moreover, according to one or the other of these suppositions, I am unable to comprehend how the deposit of older drift could be accumulated below Geneva without any admixture of clay or glacial mud having been produced.

But there is an objection which appears to me to be still more opposed to the theory of excavation.

The supporters of this theory assign to the glacier which formerly invaded our lake a force sufficiently great to enable it to remove, from a depth of 800 metres near Meillerie, all the pebbles of the older drift*. Nearer Geneva the lake is not so deep, and the glacier had still, at this point, the necessary power to scour out of the bed of the lake all those pebbles; for we know that this glacier has extended several leagues further,

* Mortillet's Theory.

and that it has passed over Mont Sion and the defile of Fort de l'Ecluse. But about a kilometre below Geneva (at the wood of La Bâtie) the older drift is visible, as I have stated, covered by the glacial deposit over a very large area. At this point one is compelled to conclude that the glacier has not had the power to remove this older drift, and that it has spread itself over it. Is it not evident that the glacier has been supposed to possess immense power above Geneva, and that there is a clear proof that it did not possess that force below the city in question? I believe, then, that the truth lies in the fact that a glacier can slide over a deposit of rolled pebbles without cutting a way through them. Consequently the ancient glaciers have not had the power to remove, near Geneva, the older drift on which they have left their traces, and, for a still greater reason, they have not had the power to remove the rolled pebbles from the bottom of the lake.

The rolled pebbles which constitute the older drift below Geneva, and which are placed beneath the glacial drift, seem to me to have been transported by the torrents which were given out by the glaciers of the Rhone and Arve when they reached the neighbourhood of our city. They have been rounded after leaving the glaciers.

This is perceptible below existing glaciers when they reach a plain. In such cases there is nearly always a certain area of deposit occupied by rolled pebbles, which are fashioned, sorted, and levelled by the torrent. The pebbles which form part of the older drift, and which are evidently derived from the Valais, have traversed the depression of the lake when it was filled with ice. They have made the journey in question in the form of erratic blocks or gravel, and were rolled only when they reached the torrent at the base of the glacier.

Subsequently, when this deposit was formed and levelled, the advancing glacier has passed over it; and, on retiring, it has left on its surface the glacial mud, the scratched pebbles and the erratic blocks which we see there even at the present day.

I have endeavoured to show that the theory of excavation was insufficient to account for the accumulation of the glacial deposits, and I ground my opinion on the weakness of the excavating power of the glacier, as is proved by the presence of the glacial drift reposing on a light deposit formed of rounded pebbles; for a still stronger reason I cannot believe that a glacier has ever excavated the basin of the lake or a valley. If these depressions had been formed by the glaciers, how shall I explain why there is no lake in the Valley of the Arve, in the valley of Chamouny, or in the Val d'Aoste? The glaciers have, nevertheless, remained for a longer period in those higher

valleys, before, during, and after the glacial epoch, than in the valley of the Lake of Geneva.

The valleys of Savoy and the Valais bear a clear relation to the structure of the mountains. They present a remarkable regularity. They are nearly all at right angles to or parallel with the general direction of the Alps. Amongst the former cases is noticed the valley of the Rhone from Martigny to the lake; that of the Dranse, which has its outlet near Thonon; that of the Arve, from Sallanche to Geneva; the valley of Lake Annecy; that of the Isère between Moutier and Albertville, and between Tigne and Bourg St. Maurice, the valley of Chapier, that of Courmayeur, &c. Amongst those which are parallel with the chain of the Alps, are the valley of the Rhone above Martigny, the valley of Chamouny, of the Allée-blanche and Entrèves, the valley of Illiers, the valley of Megève, and that of the Isère below Albertville, and between Bourg St. Maurice and Montiers.

Amongst these may we not reckon the depression in the Lake of Geneva between that town and Rolle, and which is parallel to the great anticlinal axis of the Mollasse? This axis extends from Salève to Lausanne, passing by Boisy, and is continued onwards to Bavaria*. As to the eastern parts of the lake, the direction of which is from west to east, slightly south-east, and which is considered with reason as being partly placed in a depression (*cluse*), it bears a relation to the curved form of the mountains which lie on its southern bank†. To prove this, it is necessary to enter into minute details with regard to the direction of the various parts of the chain, which would be out of place here; but I may quote an old and classical authority that nobody will call in question, and this quotation will show that the lake presents nearly the form of the mountains. "The ordinary direction of these ranges and of these valleys," says De Saussure‡, when speaking of the region lying on the right bank of the Arve (between the Arve and the Rhone), "is nearly that of the entire chain, which in our country extends from the north-east to the south-west. But this general direction varies in some places and undergoes local inflexions. One sees from the summit of the Môle that the chains of mountains, which in its

* *Bull. Soc. Géol. de France*, 1864, vol. xix. p. 928. *Archives*, 1862, vol. xiv. p. 217.

† It is not on the south bank only of the Lake of Geneva that the chains of mountains assume a circular or semicircular outline. This form is still more developed in the mountains of the left bank of the Arve than in those of the opposite bank. See, in reference to this subject, a note which I have published in the 'Reports of the British Association for the Advancement of Science,' 1860 (Trans. of Sect., p. 78).

‡ Voyages, § 280.

neighbourhood run nearly north-east, follow for a great distance the curve of the lake, and towards the frontier of the Valais take an easterly direction, as does the lake itself between Rolle and Villeneuve." This form may be recognized in my geological map of Savoy.

These great features, so characteristic of the region of the Alps which border upon us, establish an evident relation between the form and position of the lake-basin, the orography of the ground, and the cause which has elevated the mass of the Alps above the mean level of the continent.

The position of most of the Alpine lakes reveals to us, again, the relation which subsists between the mountains and the lake-basins: nearly all lie either on the borders of the Alps, or at the junction of the beds of Mollasse with hard calcareous chains. They frequently even penetrate the interior of the chains—allowing that the marshes, which are almost always at their upper end, form part of the lakes.

Such are the lakes of Geneva, of Thun, Lucerne, those of Zurich and Wallenstadt (which form only one lake in a geographical point of view), and also the Lake of Constance. In the Bavarian and Austrian Alps, again, are found the lakes of Walchen, Kochel, Schlier, Mond, Atter, Traun, &c., all on the borders of the Alps. Is this very remarkable position the result of chance? or is it not likely that in the law of the structure of the Alps there is a circumstance which has determined the formation of the lake-basins. This had been pointed out by De Saussure when, in describing the mountains lying on the right bank of the valley of the Arve, he remarked that the innermost turned their backs towards the exterior part of the Alps*, but that the outer chains turn their backs to the central chain; that is to say, that their curves are brought up on a line with the Lake of Geneva.

Since the time of De Saussure light has been thrown on the question, and the papers which you yourself have published have contributed largely to the elucidation of this subject†. It is now recognized that over the greater part of the enormous distance which separates the environs of Geneva from the eastern Alps of Austria, there is a prolonged reversal of strata, so that very often the older beds repose upon the newer. One can understand that such a great disturbance in the strata should have produced a subsidence in those which are beneath the surface by a sort of reciprocating movement (*bascule*). As regards the Lake of Geneva in particular, this reversal has been clearly pointed out on the two banks—on the northern at

* Voyages, § 281.

† Quart. Journ. Geol. Soc. 1848, vol. v. pp. 182, 195, 197, 200.

Playaux near Vevey, and on the southern at Voirons, east of Geneva. These two mountains are both situated on the borders of the Alpine chain. In order properly to grasp the relation which subsists between the overthrow of the strata along this line and the great depths of the lake, it is necessary to mark on the map the soundings of the lake by De la Beche, the positions of the crests of the Voirons, the Allinges south of Thonon, and of Playaux near Vevey *.

The principal soundings of the lake placed opposite Meillerie and Evian may likewise (and perhaps it is the most easily effected) be marked on the geological map of Savoy. Then Voirons and Playaux should be joined by a line (but not by a straight line, because the chains on the borders of the lake are curved) drawn through Calvaire (Voirons), Allinges, the point where the Alpine Macigno (M) (Affleure) descends to a level with the bed of the Dranse, at a distance of four kilometres from Thonon and the city of Evian. Such a line as this would terminate towards Playaux, passing over the northern bank of the lake between Corsier and St. Saphorin.

This course shows pretty nearly the line of the reversal of the strata situated on the flanks of the Alps; and, presenting a certain parallelism to the denudations of the different rocks traced on my geological map of Savoy, it passes through the midst of the soundings which indicate the greatest depth of the lake. Consequently this depth bears a relation to the reversal of the beds. *It is in such fractures, I am confident, that the true cause of the origin of these lake-basins is to be found.*

From a summary of these facts it may be concluded—

1st. That the Lake of Geneva deviates much from the median (central) line of the great glacier or glaciers which extended from the Rhone to the Jura.

2ndly. That these ancient glaciers not having had the power to remove the older drift below Geneva, have not been able to produce in the lake-basins what is called their excavation (*l'affouillement*). If they could not scoop out these basins, still less have they excavated the adjacent valleys which terminate in them.

3rdly. The valleys and the basins of mountain-regions are related to the cause which has given to the mountains their orographical characters, and to the strata their greater or less inclination.

* This mountain, called Plevaux or Playaux, is indicated on the Federal map by the name of Pléiades. To facilitate the indication of it on my geological map of Savoy, on which it is not marked, I should say that it lies 6 kilometres from the mouth of the stream which discharges itself into the lake between Vevey and Corsier, and 6½ kilometres from the point of Montreux.

4thly. We have seen, in fact, that, so far as the Alpine lakes generally are concerned, and as regards that of Geneva in particular, their position has been determined along a line of overthrow, or reversal of the strata. We have seen that the form of the Lake of Geneva was caused, in the eastern part, by the curvature of the mountains on its southern banks, and in its western part by its parallelism with the great anticlinal axis which traverses Switzerland.

Finally, we have remarked that the greatest depth of the Lake of Geneva lies along the line of reversed strata which occurs at the junction of the Alps with the plain. Consequently the sort of basin to which this lake belongs is not the result of a cause acting on the surface of the globe, but is what may be termed a volcanic effect (that word being used in the sense assigned to it by Humboldt), viz. the influence exerted by the interior forces of a planet on its external crust in the different stages of its cooling.

Accept, &c. &c.,

Alphonse Favre.

P.S.—Since the dispatch of my previous letter, I have read with extreme interest your Address to the Geographical Society of London, of the 23rd May, 1864, with which you have been so good as to favour me. I find in that address a clear and precise summary of the state of the question, and valuable evidences derived from many parts of the world. I perceive in it, again, with pleasure that we are of the same opinion respecting the excavation of lakes and the erosion of valleys by glaciers. You make use of several highly important arguments against that view of the question, and you have already developed the idea on which I have dwelt—viz., that the form of the Lake of Geneva is divergent from the direction of the most powerful or central portion of the glacier of the Rhone, which advanced from the Valais in the direction of Yverdun, following what I have termed the median line.

Pray, Sir, oblige me by inserting this remark at the end of my letter of the 12th of January.—A. F.

Geneva, January 22, 1865.

XXXI. *On the History of Conservation of Energy, and of its application to Physics. By* Professor Bohn*.

IT is an old experience, that great and fruitful ideas make their entrance into the world neither suddenly nor in a state of complete perfection; they generally require a certain time of de-

* Communicated by the Author.

velopment and growth, during which they may be said to belong to different persons. He who first expresses an idea with perfect clearness and exactness is commonly regarded as the discoverer, although in fact there be more than one entitled to this name. Now this rule is also applicable to the question concerning the author of the idea of transmutation of work of one kind into work of another kind. This question, however, is complicated by a circumstance which creates a difficulty in all researches on the history of force, *vis viva,* of conservation of energy, and others: these terms, as well as those of "work," "momentum of force," "momentum of activity," "dynamical effect," "mechanical power," "quantity of action," "quantity of movement," and such like, are used by different authors, frequently even by one and the same writer, in a different sense, and in so undefined a manner that a sort of translation into the more precise scientific language of the present day is required in order to clearly show the meaning of the authors.

In the Philosophical Magazine, S. 4. vol. xxviii. pp. 473, 474, Dr. C. K. Akin quotes a few sentences from Placidus Heinrich and Dr. Mohr, which he considers as the earliest statements of the "allotropy of force." It appears to me, however, that it is essentially left to the individual judgment of the reader whether he will or will not find in those sentences a certain proof of the author's firm conviction of possible transmutation of common mechanical work into heat or electricity, and *vice versâ.*

In my opinion the following remark of L. N. M. Carnot is more striking and less ambiguous than the above-mentioned quotations of Dr. Akin.

"*Vis viva* can figure either as the product of a mass and the square of its velocity, or as the product of a moving power and a length or a height. In the first case it is a *vis viva* properly called, in the second it is a latent *vis viva.*"

This seems to show that, according to Carnot's meaning, the *vis viva* consumed in raising a weight or performing other work is latent or stored up, and may be again employed in reproduction of a motion, or in the performance of another work at the cost of the first done work. This appears more evidently from another passage from Carnot:—

"All effects of propelling powers (*forces mouvantes*) may be compared to the raising of a weight to a certain height, and consequently to a *vis viva,* be it a real or a latent one."

For these statements I refer to Carnot's *Principes de l'équilibre et du mouvement*; they are surely to be found in the edition of 1803, perhaps already in the first (of 1783), whereon, not having the books at hand, I am not able at present to decide. For the same reason I was unable to quote literally; but I trust

that I have succeeded in giving an exact statement of Carnot's meaning. It must be left to the reader to decide on the importance of these sentences of Carnot.

As to the statement of Professor Faraday quoted by Dr. Akin, I can only declare my perfect concurrence with the latter.

In researches on the priority of physical truths, the preference is most justly allotted to those publications which for the first time treat a question by measure and numbers. The first quantitative study of the reciprocal transmutation of different kinds of work originated, as far as I know, from Baron Liebig. In the fourth* of his "Chemical Letters" (*Beilage zur Allgemeine Zeitung* vom 30 September 1841) he surveys the expectations which were at the time in question set on electromagnetism as a mechanical power for propelling ships, &c. In order to show the amount of clearness then pervading the ideas of the celebrated chemist on the subject in question, it would be necessary to transcribe some pages of the letter quoted. I content myself, however, with reproducing a few words:—

"Wärme, Electricität und Magnetismus sind in einer ähnlichen Beziehung einander aequivalent wie Kohle, Zink, und Sauerstoff. Durch ein gewisses Maass von Electricität bringen wir ein entsprechendes Verhältniss von Wärme oder von magnetischer Kraft hervor, die sich gegenseitig aequivalent sind. Diese Electricität kaufe ich mit chemischer Affinität, die, in der einen Form verbraucht Wärme, in der andern Electricität oder Magnetismus zum Vorschein bringt. Mit einer gewissen Summe von Affinität bringen wir ein Aequivalent Electricität hervor, gerade so, wie wir umgekehrt durch ein gewisses Maass von Electricität Aequivalente von chemischen Verbindungen zur Zerlegung bringen. Die Ausgabe für die magnetische Kraft ist also hier die Ausgabe für die chemische Affinität," &c.

Another passage is the following:—

"Aus nichts kann keine Kraft entstehen; in dem berührten Fall wissen wir, dass sie durch Auflösung (durch Oxydation) des Zinks hervorgerufen wird; allein abstrahiren wir von dem Namen, den diese Kraft hier trägt, so wissen wir, dass ihre Wirkung in einer andern Weise hervorgebracht werden kann," &c.

It is worth while to compare the first words of the latter quotation with Dr. Mayer's sentence, "ex nihilo nil fit—nil fit ad nihilum," such being the basis of his speculations in 1845.

With regard to Dr. Akin's remarks upon Huyghens (at p. 472 of No. 191 of the Philosophical Magazine), I finally beg leave to

* The twelfth in the edition of 1851 of the *Chemische Briefe*, p. 202 *et seq.*

refer to Lagrange, *Mécanique Analytique*, 3e édit. par M. Bertrand, vol. i. pp. 215, 217, or to Montucla, *Histoire des Mathématiques*, vol. iii. p. 618, and more particularly vol. iii. p. 622.

Giessen, January 28, 1865.

XXXII. *On the History of Calorescence.* *By* John Tyndall, *F.R.S., &c.**

ON the 26th of May, 1859, I presented to the Royal Society a "Note on the Transmission of Radiant Heat through Gaseous Bodies." The question had occupied me some time; but as the experimental difficulties were very great, I published the Note referred to in order to enable myself to vanquish those difficulties at my leisure. All the time at my disposal in 1859 and 1860 was devoted to the subject, and towards the end of 1860 I was so far advanced as to be able to prepare a memoir, which was presented to the Royal Society on the 10th of January, and, being chosen as the Bakerian Lecture for that year, was read on the 7th of February, 1861.

In that memoir the comparative deportment of elementary and compound gases towards radiant heat is for the first time announced. I had attempted to make radiant heat an explorer of molecular condition, and had found that the simple gases possessed a power of transmission immensely greater than that of the compound ones. In the autumn of 1861 I pursued the subject, and by purifying more perfectly the elementary gases, rendered the differences between them and the compound ones still more vast. I then turned my attention to solids and liquids, and confirmed Melloni's experiments on the diathermancy of lampblack; I also tried to render the substance more transparent to invisible heat-rays by ridding it of the hydrocarbons which attach themselves to it during its formation. I next examined the element bromine and found it eminently diathermic; I tried sulphur dissolved in bisulphide of carbon and found it still more so. I finally operated on a solution of iodine in bisulphide of carbon, and found that a layer of it, sufficiently dense to intercept completely the light of the noonday sun, offered a scarcely sensible obstacle to the passage of the invisible calorific rays†

* Communicated by the Author.

† The same *à priori* considerations which led to the discovery of the iodine, point also to red glass coloured by the *element* gold, instead of that coloured by the suboxide of copper, as most suitable for experiments on ray-transmutation. The colouring matter of the former appears to be without sensible action upon the invisible heat-rays; and the rays transmitted through it are competent to raise platinized platinum to a white heat.

This fact is thus announced in a footnote at page 67 of the Philosophical Transactions for 1862:—"A layer of bromine, sufficiently opake to intercept the entire luminous rays of a gas-flame, is highly diathermanous to its obscure rays. An opake solution of iodine in bisulphide of carbon behaves similarly. The details of these experiments shall be published in due time: they were publicly shown in my lectures many months ago.—June 13, 1862."

Turning to my published lectures on Heat as a Mode of Motion," I find at page 357 one of these experiments described in the following words:—" I cannot use iodine in a solid state, but happily it dissolves in bisulphide of carbon. I have the densely coloured liquid in this glass cell. I throw the parallel electric beam upon the screen; this solution of iodine completely cuts the light off; but if I bring my pile into the path of the beam, the violence of the needle's motion shows how copious is the transmission of the obscure rays." Turning back to page 307 of the same work, I find experiments on smoked rock-salt and black glass described as follows:—" Here is a plate of rock-salt coated so thickly with soot that the light, not only of every gas-lamp in this room, but the electric light itself is cut off by it. I interpose the plate of smoked salt in the path of the beam; the light is intercepted, but this rod enables me to find with my pile the place where the focus fell. I place the pile at this focus; you see no beam falling on the pile, but the violent action of the needle instantly reveals, to the mind's eye, a focus of heat at the point from which the light has been withdrawn."

I would ask the reader to picture any experimenter standing by a focus of invisible rays, the exposure to which, for an instant, of the face of my thermo-electric pile caused the heavy needles of a coarse galvanometer to dash against their stops. Could he escape the temptation to put his hand there? I did so fifty times, trying moreover to concentrate the radiation by pushing out my lens. The camera of my electric lamp was furnished with a concave reflector, at the centre of which stood the carbon-points whence issued the electric light. The rays were converged by a lens in front; and when the points were at their proper elevation, the focus of the lens coincided with that of the mirror. Causing the two foci to coincide, and converging the rays as much as possible, I cut off the light by the solution of iodine, and brought in succession my hand, my cheek, pieces of brown paper and of lead-foil into the dark focus. It was simply a substitution of these bodies for the face of my thermo-electric pile. But while the action on the skin was almost intolerable, I obtained neither the charring of the paper nor the fusion of the foil. I knew that a large portion of the radiant heat

of the lamp was lodged in the lens in front of it, and in the sides of the cell containing my solution of iodine; and I determined, as soon as I could turn my thoughts to it, to supplant that lens and that cell by rock-salt ones.

At that time my experiments on the action of aqueous vapour as an absorbent of radiant heat were at variance with those of an eminent natural philosopher. Respect for him, and the desire to place my results beyond the pale of doubt, determined my course of action. The autumn of 1862 was devoted to the preparation of a memoir "On the Relation of Radiant Heat to Aqueous Vapour," which was presented to the Royal Society on the 20th of November, and read on the 18th of December of that year. In the autumn of 1863 I completed my demonstration of the action of aqueous vapour on radiant heat, and at the same time executed further experiments on dissolved iodine, illustrative of its extraordinary transparency to heat-rays of ultra-red refrangibility.

My course was now cleared, and my thoughts reverted at once to the experiments which I had suspended more than two years before. I went to the Isle of Wight in March 1864 with my ideas perfectly defined and ripe for execution. While there, under circumstances which I have fully described in my article "On Negative Fluorescence," published in the January Number of the Philosophical Magazine, I received a note from Dr. Akin, a Hungarian gentleman who had arrived in this country in the early part of 1862, and who was personally unknown to me. His name had been mentioned to me by Professor Stokes; I had read an article from his pen published in the 'Saturday Review,' and he now forwarded to me a proof of a Report presented by him to the British Association assembled at Newcastle in 1863. In this Report three experiments were proposed: two of them were impracticable, but they nevertheless showed ability, while the third seemed to offer a promise of real success. These were not the experiments that I had proposed to make; I did not intend to experiment on the sun, for his beams are unattainable where I work; I intended to operate with the radiant source with which the constant practice of the previous eleven years had rendered me familiar, and to realize ideas of which the origin has been indicated above, but which, with the caution of a man who knows the difference between conception and performance, I did not publish in detail. The reader will now be in a position to appreciate the use which Dr. Akin has made of the following note written to him at the time:—

"Clarendon Hotel, Chale, Isle of Wight,
Wednesday.

"My dear Sir,—I have read the proof which you have been kind enough to send me with extreme interest. Being a truant from the

Sections at Newcastle, I did not know that you had brought the subject forward there. Professor Stokes, however, informed me that you were engaged on the question, and I had similar information from the 'Saturday Review.'

"There is certainly a most singular coincidence in the thoughts which have passed through both our minds on some of the points in question. As to the possibility of converting the Herschellic into the Newtonic rays I do not entertain a doubt; and indeed the piece of work which I had set before me for attack on my return from this place was a series of experiments on this very subject.

"I have devised a very perfect means of sifting the Herschellic from the Newtonic rays, and with the latter alone I have strong hopes of being able to produce incandescence. But you have entered this field before me, and although the question lies directly in the way of my own inquiries I should like to do in the matter what would be most pleasing to you. I shall be in town on Saturday next, at the Royal Institution at noon; could you not look in upon me there?

"Yours very sincerely,

"John Tyndall."

In the December Number of the Philosophical Magazine, p. 559, Dr. Akin employs the following language with reference to this letter:—"I must be allowed to express here a doubt whether Sir Humphry Davy, for example, or any other predecessor of Professor Tyndall at the Royal Institution, having read in a public print that two persons were engaged in making researches on a certain subject with the aid and sanction of the British Association, would have chosen 'that very subject' for 'attack' some little time after." I, on the other hand, may be permitted to express the assured conviction that Sir Humphry Davy would never have given Dr. Akin the chance of garbling his language as he has here garbled mine. The reader will compare the words "attack" and "that very subject," as held in the claws of his inverted commas, with the same words as they occur in my note, and draw his own conclusions as to the temper of the quoter.

The fundamental error of Dr. Akin consists in his interpreting the attitude assumed by me in the foregoing note, as one which, under the circumstances, he had a right to expect. What most men would have regarded as an act of free courtesy on my part, Dr. Akin interprets as an acknowledgment of his inordinate claims. He does not seem to understand why, of my own free will, I conceded to him a position which I should unquestionably have refused had it been demanded as a right. As I have already stated to him, "there was nothing in the circumstances of the case to prevent me from working at the subject, restricting myself to a due reference to his labours." Had I been aware that my note had excited his "astonishment," I should have acted

very differently towards him. Had he urged "the authority of the British Association," I should have informed him that I could not recognize its authority in such a matter. At the same time I should have known that the eminent leaders of that body would never countenance Dr. Akin in the attempt to shut me out of a field of inquiry which I had virtually entered before him, and which consideration for him as a stranger had alone caused me to relinquish. To render this assurance doubly sure, I wrote to General Sabine, who knows more about the constitution of the British Association than any other living man, and who, with the kindness which I have always experienced at his hands, promptly wrote to me the following reply:—

"Dear Tyndall,—Viewing your inquiry simply as an administrative question, Dr. Akin's Report on the Transmutation of Spectral Rays in the British Association Reports for 1863 indicated a very wide field of experimental researches. In aid of these the General Committee made a grant of £45 to Mr. Griffith and himself. A part of this grant was expended in apparatus to be employed by those gentlemen in the spring or early summer of 1864, the results of which were designed by Dr. Akin to be presented by himself to the Meeting of the British Association at Bath in the September of that year. For this purpose Dr. Akin went to Oxford to join Mr. Griffith, but from some unexplained cause their purpose was frustrated, and 1864 passed without the experiments being made. The grant, however, did not lapse, as part of it had been expended in apparatus, which it appears to be still Dr. Akin's intention to employ (though not at Oxford where there is sunshine, or in London where there is no sunshine suitable for the purpose). Such, so far as I can collect them, is a statement of the facts.

"The object of the British Association in making such grants is to aid the progress of science, by enabling those, who may be so disposed, to make experiments, but by no means to retard its progress by discouraging others from approaching the same ground. One cannot read, I think, Dr. Akin's Report in 1863 without the impression that there is work enough in the subject for a dozen at least of active experimenters. He may have had an advantage (and properly used it is a great one) in meditating longer than others on the laurels that may be gathered, but he who desires to be foremost in the field must be 'prompt in action' where there are so many competitors.

"Always truly yours,

"Edward Sabine."

I also wrote to Sir William Armstrong on the same subject. As President of the British Association at Newcastle, Sir William Armstrong does not think himself entitled now to speak, but as a private individual he states that the British Association "could not, even if they proposed anything so foolish, confer upon Dr. Akin any exclusive privileges in the matter."

It was in answer to the express desire of Dr. Akin to see me that I informed him at the end of my note from Chale that I should be in town on the following Saturday. He came. I have since ransacked my memory for the details of our interview; and though I cannot recall the whole of them, the essential ones are perfectly clear to me, while the *result* is as vivid to my mind as the occurrence of yesterday. Without waiting to obtain the knowledge of Dr. Akin's character that ten minutes' conversation might have given, I remarked that although he and I had reached the same point by independent routes, he was the first to publish definitely his ideas, and that, therefore, I would not interfere with him. I stated that although my plans were prepared, I was perfectly willing to suspend them. Dr. Akin's reply was that he was delighted to find the subject taken up, and was only anxious to see it prosecuted to a successful issue. Of these words I have the most distinct recollection. I did not know at the time that the note written to him on the previous Wednesday had excited his "astonishment"; nor did I know that he had come, as he has since informed me, expecting that I would make him an offer that we should work together; but pleased by his apparent frankness, on the spur of the moment I used these words:—"We might perhaps attack it together." I use the phrase "spur of the moment" because it strictly represents the fact, but they who know me best will be the last to believe me capable of retreating from a position assumed even on the spur of the moment. And had Dr. Akin agreed to my proposal, no matter what consequent penalties it might have involved—and they, I doubt not, would have been great—I should have carried it out to the letter. But he did *not* agree to my proposition. He said that he had associated himself with Mr. Griffith at Oxford, and that he was compelled to fulfil his engagement with that gentleman.

Let me here remark that neither Dr. Akin nor myself professes to remember the whole of what occurred during this interview; and when he spoke of his being bound to Mr. Griffith, some conversation followed, the precise terms of which I do not recollect; but it drew from Dr. Akin a proposition, which I *do* remember perfectly well. I will allow him to supply what I have here forgotten. In a letter written to me more than three months ago, he affirms that when he declared himself bound to Mr. Griffith at Oxford, I rejoined:—"Can we not make it a triple alliance?" His reply was, "No, leave us the sun at Oxford, but as regards experiments on artificial rays, I shall be happy to cooperate with you separately." Of the words "triple alliance" I have no recollection, but they are so like what I should use under the circumstances, that I do not doubt the correctness of Dr.

Akin's memory regarding them. But I do remember his proposal to work with me in London and with Mr. Griffith at Oxford, and it caused me to pause. Neither in its tone nor in its terms did it please me, and to it, by neither word nor sign, did I ever show the slightest symptom of assent.

In his last published account of this conversation Dr. Akin omits all reference to my proposal and his refusal of a "triple alliance," and represents me as accepting his counterproposition without reservation or qualification. "To this conclusion Professor Tyndall cheerfully agreed, observing that he had from the first intended to make experiments on the electric light; that he was consequently quite satisfied with the arrangement proposed." Though there seems *now* no shade of doubt upon Dr. Akin's mind regarding my acceptance of his offer, still I think there was a slight shade upon it four months ago. In a letter written to me on the 1st of last November, after reminding me of his offer to cooperate with me in London, leaving the sun to himself and Mr. Griffith at Oxford, he continues thus:—"If there was no 'closing' with any offer, it must have been on your part, and as regards this last-mentioned offer, which I made in answer to your own"*. This passage, I submit, indicates a condition of mind and memory somewhat different from that which prompted the unqualified assertion "to this condition Professor Tyndall cheerfully agreed."

When I wrote to Dr. Akin from the Isle of Wight, my desire and intention was to allow him ample scope for the realization of his ideas, and during our conference this intention was

* Here is the extract *in extenso*: it is taken from Dr. Akin's second letter to me, the word "closing" being placed between inverted commas, because in my reply to Dr. Akin's first letter I had reminded him that he had not "closed" with my proposal:—

"Dear Sir,—I have just received your letter of today, and intend to shortly reply to such portions as seem to me to require it.

"As regards the way in which your attention was turned to the subject which is in question, of course you are the best as well as the only authority, and I have no intention of challenging any statement on your part, deliberately made, and where there are no facts within my knowledge proving that your recollection misleads you. Such, however, I believe to be the case in reference to what occurred at the Royal Institution in April, in the course of our conversation. You then spoke of a 'triple alliance'[a] (those were the words you used). I replied, 'Leave the sun to us at Oxford,' but added that, as regarded experiments on artificial sources of rays (the expressions, I believe, were 'the other two experiments,' meaning those described in my paper), I should be happy to cooperate with you separately. If there was 'no closing' with any offer, it must have been on your part, and as regards this last-mentioned offer, which I made in answer to your own."

"[a] That was after I had said that I was bound to Mr. Griffith."

revived. I resolved then and there not to interfere with Dr. Akin, to suspend the experiments I had planned, and leave the subject entirely in his hands throughout the summer. During the months of April, May, and June my assistant was employed, under my direction, on an investigation which he afterwards published in the August Number of the Philosophical Magazine, while I occupied my leisure with other matters. The naked facts stand thus:—On Wednesday the 30th of March, I write to Dr. Akin stating that I had devised a series of experiments to be undertaken on my return to town. On the very day of my arrival the execution of these experiments is postponed, and not a word is heard of them until the end of October.

Meanwhile Dr. Akin had pursued his experiments at Oxford, and had sent in the following Report of his performances to the President of the Physical Section of the British Association. The document is dated from the Cavendish Club, London, 15th September, 1864.

"I am sorry to have to state that, owing to the scarcity of clear sunshine at Oxford last summer, and to Mr. Griffith being mostly unable to give his time to our work when the weather happened to be favourable to it, the experiments were left in August unfinished. I had the greater reason to feel disappointed at such a conclusion, as the methods and apparatus devised for the purpose appeared, from preliminary trials, competent to effect, in part at least, the proposed object, and as our preparations for the more decisive experiments were in a very forward state. Meanwhile I have received fresh testimony from several esteemed quarters to the importance of the research thus partially accomplished, which makes me feel yet deeper regret at seeing the matter after two years' labour, and nearly three years' thought and trouble—some of it unexpectedly galling—still in abeyance."

After the Bath Meeting of the British Association, Dr. Akin also wrote thus to the President of the Association:—"I now beg leave to state that, after the experience of the last two years, it would be a hopeless undertaking for me to continue at Oxford the experiments begun there. Moreover I am not sure whether, after the end of the present year, I shall be able to give my attention any longer to scientific researches." Supposing Dr. Akin to be perfectly satisfied in his own mind that he had entered into a definite engagement with me, does it not seem odd that he should not have hinted at this engagement, either in his Report or in his letter? When he stated that he had abandoned hope in Oxford, it would surely have been natural to add that he had *not* abandoned it in London. The fact seems to be that Dr. Akin had no clear con-

sciousness at the time that such an engagement existed, and that anger now supplies him with a power of memory which in his calmer moments he did not enjoy.

I went to Switzerland early in July, and from Pontresina wrote to my assistant, describing the apparatus I had intended to employ, and desiring him to have it prepared for me. One portion of it consisted simply of the substitution of a rock-salt lens for the glass one of the electric lamp, and a rock-salt cell for the glass one that I had used in 1862. With this arrangement I executed the experiments "on luminous and obscure radiation" described in the November Number of the Philosophical Magazine. Nay, abandoning the rock-salt altogether, and employing a glass lens of somewhat shorter focus than that made use of in 1862, with a battery of sixty cells, I obtained all the results with the electric lamp there described. In not the slightest particular does this arrangement differ from that which I had employed two years and three-quarters previously. We have the same source of rays, the same mode of convergence, and the same absorbent to intercept the luminous portion of the radiation. And yet it is from this, my own ground, which I had taken up practically before he had perused the "little German treatise which first taught him what had been done" on the subject of fluorescence, that Dr. Akin would fence me out.

Experiments on combustion could not decide the question of ray-transmutation. "Intimately connected" they assuredly were,—so intimately, indeed, that the man who could neglect to pass immediately from the one to the other, would be unfit for the vocation of a natural philosopher. Was I, then, out of consideration for Dr. Akin, to keep my platinum-foil or my silver-leaf away from the focus of dark rays, lest it should become incandescent there? My duty to science ruled otherwise; and as no bond or promise existed to contravene that duty, I resolved not to let the autumn pass as Dr. Akin had the summer, without making some attempt to realize *my* ideas. He had already tried, failed, and announced his failure. Still, however, wishing to treat him with the utmost consideration, I resolved, while forwarding the work of science, to leave the chance of prior publication open to Dr. Akin. I determined to hold back whatever results I might obtain until he would be either able to precede me, or could so far merge his individual interests in the larger interests of science, as to be willing to see the results published. This he deems an unusual proceeding on my part, and so it assuredly was. But from first to last my proceeding towards him was unusual. In relation to any other man it would have been

more than fair; in relation to him it has proved to be worse than useless.

Late in the day of the 29th of October, I obtained the incandescence of platinum foil by the dark rays of the electric light. The layer of iodine, however, was too thin to wholly intercept the light, while the battery had been enfeebled by long-continued previous action. On the following morning it was freshly charged, the number of cells being increased from fifty to sixty. Removing from the back of the camera the little reflector which I had made use of in 1862, I mounted it on a retort-stand, and with it converged the rays from the lamp. The mirror was silvered behind, and the reflected rays had therefore to pass to and fro across a plate of glass of considerable thickness. Notwithstanding this, however, and the employment of a much thicker layer of dissolved iodine, the platinum, when placed in the dark focus of the mirror, became vividly incandescent. Almost immediately after the performance of this experiment I found Dr. Akin in the library of the Royal Institution. Without a moment's hesitation I invited him to accompany me to the laboratory, and there almost simultaneously with myself he saw the result which I had obtained*.

Not one word of remonstrance or complaint escaped him on this occasion; not a syllable to indicate that he believed me in any way pledged to him. He made some critical remarks as to the experiment being made in daylight. He was moreover struck with the thickness of the layer of iodine employed, observing that the layer used by him at Oxford was a thin one, which did not cut off the light. He finally expressed himself delighted to see the experiment, and we quitted the laboratory together. I walked with him upstairs, and there at the eleventh hour expressed my desire not to interfere with him, and to leave, if he wished it, the subject still in his hands. His reply was, "It will all depend on the manner of publication." Knowing my own willingness to do him the amplest justice in any publication of mine, I responded, "You shall see my manuscript before I publish."

My intention when I parted from Dr. Akin was to give a brief account of the experiment at the first meeting of the Royal Society, but I had not definitely made up my mind. On the evening of the day to which I now refer I thought and spoke of associating myself with Dr. Akin for the further prosecution

* Dr. Akin had informed me that he was collecting materials for a "History of Force," and needed access to a good library. Deeming his work both useful and honourable, to enable him to prosecute it I introduced him to the libraries both of the Royal Institution and the Athenæum Club.

of the inquiry. On the following Monday, however, I received a letter from this gentleman which entirely changed the aspect of affairs, and in which he informed me that, on thinking over the experiment I had showed him, his mind had become greatly agitated. He referred to our interview in April, and to what he asserted had occurred between us since. Like his last article in this Journal, that letter consisted of a single broken fibre of truth in a tissue of error. I then went through the assertions of Dr. Akin one by one, pointed out where they were exaggerated, where he had unduly intensified language, and where his statements were, as I believed, at direct variance with facts. His last paper furnishes some illustration of the intensification of language which I then sought to correct. In my reply to his intimation that I had appropriated his ideas, I stated that "my work would have been far more completely done by this time had he never existed." Dr. Akin's version of this statement of a fact is this:—"Professor Tyndall, who is charitable enough to express an implied wish for my non-existence, will no doubt be gratified to learn that the experience I have met with at his hands has acted upon me in the manner he would seem to desire."

It was sufficiently evident from Dr. Akin's correspondence that he was very much disappointed. He had been "greatly depressed and discouraged in mind" at Newcastle. "An indelible mark of pain" had been left upon his memory by the treatment he received at Oxford; and now his misfortunes culminated on finding, in London, the goal at which he aimed attained by another. Still that other, though sorely irritated, was less anxious to avail himself of his success than to show Dr. Akin "how lightly he valued the whole matter when a question of good faith came into play." The letter which contained my corrections of Dr. Akin's misapprehensions concluded thus:—

"And now for a few concluding words:—I cannot help expressing my deep regret that you did not write to me in a different manner. I should have gone almost any length to gratify you, had you adopted another tone; and I shall prove my sincerity in writing thus, by doing that which still remains in my power to do. I have to say, then, that from this 3rd of November 1864 to the 3rd of November 1865 I shall not make known, publicly or privately, any experiments on 'ray-transmutation.' Nearly seven months ago I was, as you know, ready to take this subject up. Out of deference to you I did not do so; so that at the termination of the period referred to I shall have held back for nearly nineteen months. That you will be able to release me long before, by the actual performance of this experiment, is almost beyond a doubt; and in this event I hope to muster sufficient greatness of heart not to envy you."

Dr. Akin has the assurance to upbraid me with having, in the face of this promise, seized the first opportunity to publish my results. My promise was given in order to enable him to perform honest work, and not to indulge in aggression.

I halted that he might move on. The arrangement left open to him another summer, during which he might renew the investigation that had broken down at Oxford, with the assurance, moreover, that the object at which he aimed was no longer a doubtful one. Not satisfied with this arrangement, he continued to complain, and I was at length obliged to end a profitless and apparently interminable correspondence with these words:—"I have now only to add, that when you have so far overcome the agitation of mind to which you so frequently refer, as to be able to look at this question with the eyes and mind of a philosopher, I shall be happy to communicate with you. But no possible good can arise from a continuance of this correspondence. Hence, as far as I am concerned, it is for the present at an end."

Dr. Akin then wrote to me his fifth letter, and ended it by the statement that, although he could scarcely allow himself to hope much for the future, he wished, nevertheless, to say that whenever I should be pleased again to communicate with him he would be happy to hear from me. This letter was dated from Harley Street, November 9, 1864.

For nearly three weeks I heard nothing from or of Dr. Akin, but he meanwhile was not idle. Silently, and without a word of warning to me, he concocted his "Note on Ray-transmutation" for the December Number of the Philosophical Magazine. After the completion of that article, and twenty-four hours previous to its publication, he wrote to me commencing his letter with "Dear Sir," and ending it with "Believe me yours very faithfully." On the very day of its publication he sent me a second note with somewhat similar termini; but neither of them breathed a whisper of his attack. During my correspondence with Dr. Akin I availed myself from time to time of the wisest advice within my reach; and the verdict of my advisers was now prompt and emphatic. From the legal, the scientific, and the general world, I heard the unanimous opinion that Dr. Akin's act of war cancelled every treaty subsisting between us, and rendered the act of publication a duty to science as well as to myself.

To sum up:—

1. Dr. Akin affirms that I have interfered with an investigation which he had originated and to which he had secured an exclusive right.

I assert, on the contrary, that my inquiries have been the direct result of labours commenced before Dr. Akin was heard of in this or any other field of scientific inquiry; and that even if

they had not been, the assumption of any such right of exclusion is inadmissible.

2. Dr. Akin affirms that he closed with alacrity with a proposition of mine to work out, in conjunction with him, a certain subject.

But Dr. Akin admits that he did not "close" with my first proposition, on account of his tie to Mr. Griffith. He further admits that he declined the "triple alliance" between Mr. Griffith, Dr. Akin, and myself, which was next suggested by me. Finally, he produces no evidence to show that I proposed or accepted any other arrangement; and I categorically deny that any other arrangement was proposed or accepted by me.

3. Dr. Akin's words imply that I have availed myself of his ideas or suggestions, to anticipate his results. I answer thus:—

It has been shown in the foregoing article that early in the year 1862 I formed foci of invisible rays, and also illustrated in my public lectures the heat of such foci; that I employed for this purpose the condensed rays of the electric light, and intercepted the luminous part of the radiation by an absorbent of extraordinary properties discovered by myself.

Sir William Herschel had tried to render the ultra-red rays of the solar spectrum visible by condensation, having first separated them, by prismatic analysis, from the visible rays. But the idea of forming intense foci of invisible rays by concentrating the *total* radiation of a luminous source, and removing the visible rays by an absorbent, is, I believe, entirely mine.

4. In the autumn of 1863 Dr. Akin proposed to obtain an intense focus of invisible rays by converging the sun's beams and intercepting the luminous part of the radiation by absorbents which he did not name. In making this proposition, Dr. Akin had, without a word of acknowledgment, appropriated an idea which I had in substance enunciated and applied more than a year and a half previously.

5. The only particular in which Dr. Akin was in advance of me—and this not in thought, but in publication—was that he was the first to express the belief that the dark rays of the sun, when detached from the luminous ones by my method, would be competent to raise platinum-foil to incandescence; and that if they did so we should have for the extra-red end of the spectrum phenomena analogous to those of fluorescence.

6. In consequence of the precision with which he had enunciated this idea, and also in part on account of his being a foreigner, working, as I conceived, against difficulties in this country, I somewhat Quixotically permitted him during the whole of last summer to pursue his idea undisturbed. He had tried, failed, and reported his failure to the British Association

assembled at Bath, before my experiments on calorescence had commenced.

If I have not waited longer before publishing the results of my experiments, as I once proposed to do, it is because by his unwarranted and unprovoked attacks, Dr. Akin has forfeited all claim to the position in which I had voluntarily, but unwisely, placed him.

Royal Institution, February.

[The Editors have received a letter from Dr. Akin, in which he begs to enter a public and formal protest against the note appended to his communication in the last Number of the Magazine by W. F., and claims to have the last word in this discussion. As Dr. Akin commenced the controversy in the December Number, and in that of February has had every opportunity of placing all the evidence he deemed necessary before our readers, we cannot admit the justice of his claim, and abide by the decision already given. —The Editors.]

XXXIII.—*Note on* Lobatschewsky's *Imaginary Geometry*.

By A. Cayley, *Esq.**

WRITING down the equations

$$\frac{1}{\cos a'} = \cos a = \frac{\cos A + \cos B \cos C}{\sin B \sin C},$$

$$\frac{1}{\cos b'} = \cos b = \frac{\cos B + \cos C \cos A}{\sin C \sin A},$$

$$\frac{1}{\cos c'} = \cos c = \frac{\cos C + \cos A \cos B}{\sin A \sin B},$$

where A, B, C are real positive angles each $< \frac{1}{2}\pi$: *first*, if $A + B + C > \pi$, then a, b, c are real positive angles each less than $\frac{1}{2}\pi$ (this is in fact the case of a real acute-angled spherical triangle), but a', b', c' are pure imaginaries of the form $p'i$, $q'i$, $r'i$ (where p', q', r' are real positive quantities); and *secondly*, if $A + B + C < \pi$, then a, b, c are pure imaginaries of the form pi, qi, ri (where p, q, r are real positive quantities), but a', b', c' are real positive angles each less than $\frac{1}{2}\pi$. Hence assuming $A + B + C < \pi$, and writing ai, bi, ci in place of a, b, c, the system is

$$\frac{1}{\cos a'} = \cos ai = \frac{\cos A + \cos B \cos C}{\sin B \sin C},$$

$$\frac{1}{\cos b'} = \cos bi = \frac{\cos B + \cos C \cos A}{\sin C \sin A},$$

* Communicated by the Author.

$$\frac{1}{\cos c'} = \cos ci = \frac{\cos C + \cos A \cos B}{\sin A \sin B},$$

which equations (if only we write therein $\frac{\pi}{2} - a'$, $\frac{\pi}{2} - b'$, $\frac{\pi}{2} - c'$ in place of a', b', c' respectively) are in fact the equations given under a less symmetrical form in the curious paper "Géométrie Imaginaire" by N. Lobatschewsky, Rector of the University of Kasan, *Crelle*, vol. xvii. (1837) pp. 295–320. The view taken of them by the author is hard to be understood. He mentions that in a paper published five years previously in a scientific journal at Kasan, after developing a new theory of parallels, he had endeavoured to prove that it is only experience which obliges us to assume that in a rectilinear triangle the sum of the angles is equal to two right angles, and that a geometry may exist, if not in nature at least in analysis, on the hypothesis that the sum of the angles is less than two right angles; and he accordingly attempts to establish such a geometry, viz. a, b, c being the sides of a rectilinear triangle, wherein the sum of the angles $A + B + C$ is $< \pi$, and the angles a', b', c' being calculated from the sides by the formulæ

$$\cos a' = \frac{1}{\cos ai}, \quad \cos b' = \frac{1}{\cos bi}, \quad \cos c' = \frac{1}{\cos ci}$$

(I have, as mentioned above, replaced Lobatschewsky's a', b', c' by their complements): the relation between the angles A, B, C and the subsidiary quantities a', b', c' which replace the sides, is given by the formulæ

$$\frac{1}{\cos a'} = \frac{\cos A + \cos B \cos C}{\sin B \sin C},$$

$$\frac{1}{\cos b'} = \frac{\cos B + \cos C \cos A}{\sin C \sin A},$$

$$\frac{1}{\cos c'} = \frac{\cos C + \cos A \cos B}{\sin A \sin B}.$$

I do not understand this; but it would be very interesting to find a *real* geometrical interpretation of the last-mentioned system of equations, which (if only A, B, C are positive real quantities such that $A + B + C < \pi$; for the condition, A, B, C each $< \frac{1}{2}\pi$, may be omitted) contains only the *real* quantities A, B, C, a', b', c'; and is a system correlative to the equations of ordinary Spherical Trigonometry.

It is hardly necessary to remark that the equation

$$\frac{1}{\cos a'} = \cos ai$$

is Jacobi's imaginary transformation in the Theory of Elliptic Functions. See, as to this, my paper "On the Transcendent $\mathrm{gd}.u=\frac{1}{i}\log\tan\left(\frac{\pi}{4}+\frac{1}{2}ui\right)$," Phil. Mag. vol. xxiv. (1862) pp. 19–22.

Cambridge, January 21, 1865.

XXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 157.]

December 22, 1864.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair.

THE following communication was read:—

"On a Method of Meteorological Registration of the Chemical Action of Total Daylight." By Henry E. Roscoe, B.A., F.R.S.

The aim of the present communication is to describe a simple mode of measuring the chemical action of total daylight, adapted to the purpose of regular meteorological registration. This method is founded upon that described by Prof. Bunsen and the author in their last Memoir* on Photochemical Measurements, depending upon the law that equal products of the intensity of the acting light into the times of insolation correspond within very wide limits to equal shades of tints produced upon chloride-of-silver paper of uniform sensitiveness—light of the intensity 50, acting for the time 1, thus producing the same blackening effect as light of the intensity 1 acting for the time 50. For the purpose of exposing this paper to light for a known but very short length of time, a pendulum photometer was constructed; and by means of this instrument a strip of paper is so exposed that the different times of insolation for all points along the length of the strip can be calculated to within small fractions of a second, when the duration and amplitude of vibration of the pendulum are known. The strip of sensitive paper insolated during the oscillation of the pendulum exhibits throughout its length a regularly diminishing shade from dark to white; and by reference to a Table, the time needed to produce any one of these shades can be ascertained. The unit of photo-chemical action is assumed to be that intensity of light which produces in the unit of time (one second) a given but arbitrary degree of shade termed the standard tint. The reciprocals of the times during which the points on the strip have to be exposed in order to attain the standard tint, give the intensities of the acting light expressed in terms of the above unit.

By means of this method a regular series of daily observations can be kept up without difficulty; the whole apparatus needed can be packed up into small space; the observations can be carried on without regard to wind or weather; and no less than forty-five separate determinations can be made upon 36 square centimetres of sensitive paper. Strips of the standard chloride-of-silver paper tinted in the pendulum photometer remain as the basis of the new mode

* Phil. Trans. 1863, p. 139.

of measurement. Two strips of this paper are exposed as usual in the pendulum photometer : one of these strips is fixed in hyposulphite-of-sodium solution, washed, dried, and pasted upon a board furnished with a millimetre-scale. This fixed strip is now graduated in terms of the unfixed pendulum strip by reading off, by the light of a soda-flame, the position of those points on each strip which possess equal degrees of tint, the position of the standard tint upon the unfixed strip being ascertained for the purpose of the graduation. Upon this comparison with the unfixed pendulum strip depends the subsequent use of the fixed strip. A detailed description of the methods of preparing and graduating the strips, and of the apparatus for exposure and reading, is next given. The following conditions must be fulfilled in order that the method may be adopted as a trustworthy mode of measuring the chemical action of light : —

1st. The tint of the standard strips fixed in hyposulphite must remain perfectly unalterable during a considerable length of time.

2nd. The tints upon these fixed strips must shade regularly into each other, so as to render possible an accurate comparison with, and graduation in terms of, the unfixed pendulum strips.

3rd. Simultaneous measurements made with different strips thus graduated must show close agreement amongst themselves, and they must give the same results as determinations made by means of the pendulum photometer, according to the method described in the last memoir.

The fixed strips are prepared in the pendulum apparatus, and afterwards fixed in hyposulphite of sodium. A series of experiments is next detailed, carried out for the purpose of ascertaining whether these fixed strips undergo any alteration by exposure to light, or when preserved in the dark. Two consecutive strips were cut off from a large number of different sheets, and the point upon each at which the shade was equal to that of the standard tint was determined. One half of these strips were carefully preserved in the dark, the other half exposed to direct and diffuse sunlight for periods varying from fourteen days to six months, and the position of equality of tint with the standard tint from time to time determined. It appears, from a large number of such comparisons, that in almost all cases an irregular, and in some cases a rapid fading takes place immediately after the strips have been prepared, and that this fading continues for about six to eight weeks from the date of the preparation. It was, however, found that, after this length of time has elapsed, neither exposure to sunlight nor preservation in the dark produces the slightest change of tint, and that, for many months from this time, the tint of the strips may be considered as perfectly unalterable.

The value of the proposed method of measurement entirely depends upon the possibility of accurately determining the intensities of the various shades of the fixed strip in terms of the known intensities of the standard strips prepared in the pendulum photometer. The author examines this question at length, and details two methods of graduating the fixed strips, giving the results obtained in several series of determinations, in order that the amount of experi-

mental error may be estimated. Curves exhibiting the graduation of several strips are also given; and from these the author concludes that the determinations agree as well as can be expected from such photometric experiments, the mean error between the positions 40 and 80 min. on the strip in one series of graduations not exceeding 1 per cent. of the measured intensity. To each fixed strip a Table is attached, giving the intensity of the light which must act for 1 second upon the standard paper, in order to produce the tints at each millimetre of the length of the strip.

The methods of exposure and reading are next described. The exposure of the paper is effected very simply by pasting pieces of standard sensitive paper upon an insolation band, and inserting the band into a thin metal slide having a small opening at the top and furnished with a cover, which can be made instantly to open or close the hole under which the sensitive paper is placed. When one observation has thus been made, and the time and duration of the insolation noted, the remaining papers can be similarly exposed at any required time; and thus the determinations can be very easily carried on at short intervals throughout the day.

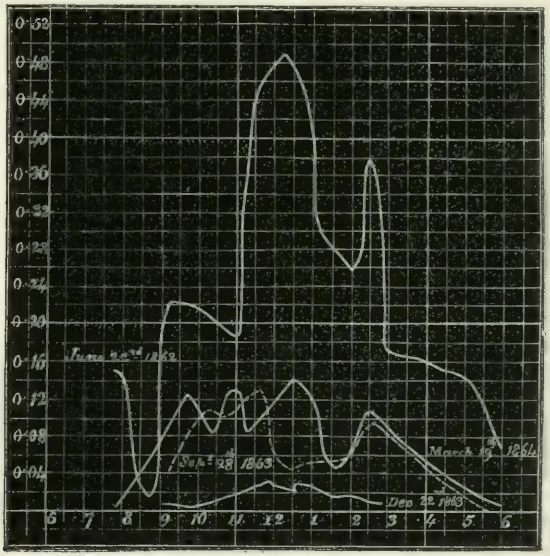
The reading-instrument consists of a small metallic drum, furnished with a millimetre scale, and upon which the graduated strip is fastened. The drum turns upon a horizontal axis, and the insolation band, with the exposed papers upon it, is held against the graduated strip, so that by moving the drum on its horizontal axis the various shades of the strip are made to pass and repass each of the papers on the insolation band, and the points of coincidence of tint on the strip and on each of the exposed papers can be easily ascertained by reading off with the monochromatic soda-flame.

In the next section of the paper the author investigates the accuracy and trustworthiness of the method. This is tested in the first place by making simultaneous measurements of the chemical action of daylight by the new method and by means of the pendulum photometer, according to the mode described in the last memoir, upon which the new method is founded. Duplicate determinations of the varying chemical intensity thus made every half-hour on four separate days give results which agree closely with each other, as is seen by reference to the Tables and figures showing the curves of daily chemical intensity which are given in the paper. Hence the author concludes that the unavoidable experimental errors arising from graduation, exposure, and reading are not of sufficient magnitude materially to affect the accuracy of the measurement. As a second test of the trustworthiness and availability of the method for actual measurement, the author gives results of determinations made with two instruments independently by two observers at the same time, and on the same spot. The tabulated results thus obtained serve as a fair sample of the accuracy with which the actual measurement can be carried out; and the curves given represent graphically the results of these double observations. From the close agreement of these curves, it is seen that the method is available for practical measurement.

In order to show that the method can be applied to the purposes

of actual registration, the author gives the results of determinations of the varying intensity of the chemical action of total daylight at Manchester on more than forty days, made at the most widely differing seasons of the year. These measurements reveal some of the interesting results to which a wide series of such measurements must lead. They extend from August 1863 to September 1864; and Tables are given in which the details of observations are found, whilst the varying chemical intensity for each day is expressed graphically by a curve.

Figure showing curves of daily chemical intensity at Manchester, in spring, summer, autumn, and winter.



As a rule, one observation was made every half-hour; frequently, however, when the object was either to control the accuracy of the measurement or to record the great changes which suddenly occur when the sun is obscured or appears from behind a cloud, the determinations were made at intervals of a few minutes or even seconds.

Consecutive observations were carried on for each day for nearly a month, from June 16th to July 9th, 1864; the labour of carrying out these was not found to be very great, and the results obtained are of great interest. By reference to the Tables, it is seen that the amount of chemical action generally corresponds to the amount of cloud or sunshine as noted in the observation; sometimes, however, a considerable and sudden alteration in the chemical intensity occurred when no apparent change in the amount of light could be noticed by the eye. The remarkable absorptive action exerted upon the chemically active rays by small quantities of suspended particles of water in the shape of mist, or haze, is also clearly shown. For the purpose of expressing the relation of the sums of all the various

hourly intensities, giving the daily mean chemical intensities of the place, a rough method of integration is resorted to: this consists in determining the weights of the areas of paper inscribed between the base-line and the curve of daily intensity, that chemical action being taken as 1000 which the unit of intensity would produce if acting continuously for twenty-four hours. The remarkable differences observed in the chemical intensity on two neighbouring days is noticed on the curves for the 20th and 22nd of June 1864: the integrals for these days are 50·9 and 119, or the chemical actions on these days are in the ratio of 1 to 2·34.

The chemical action of light at Manchester was determined at the winter and summer solstices, and the vernal and autumnal equinoxes: the results of these measurements are seen by reference to the accompanying curves. The integral for the winter solstice is 4·7, that of the vernal equinox 36·8; that of the summer solstice 119, and that of the autumnal equinox 29·1. Hence, if the chemical action on the shortest day be taken as the unit, that upon the equinox will be represented by 7, and that upon the longest day by 25.

The results of simultaneous measurements made at Heidelberg and Manchester, and Dingwall and Manchester, are next detailed.

From the integrals of daily intensity the mean monthly and annual chemical intensity can be ascertained, and thus we may obtain a knowledge of the distribution of the chemically acting rays upon the earth's surface, such as we possess for the heating rays.

January 26, 1865.—Major-General Sabine, President, in the Chair.

The following communication was read:—

"Researches on Solar Physics.—Series I. On the Nature of Solar Spots." By Warren De la Rue, Ph.D., F.R.S., Balfour Stewart, A.M., F.R.S., &c. and Benjamin Loewy, Esq.

After giving a short sketch of the history of their subject, the authors proceed to state the nature of the materials which had been placed at their disposal In the first place, Mr. Carrington had very kindly put into their hands all his original drawings of sun-spots, extending from November 1853 to March 1861. In the next place, their materials were derived from the pictures taken by the Kew heliograph. A few pictures were taken by this instrument at Kew Observatory in the years 1858 and 1859. In July 1860 it was in Spain doing service at the total eclipse. In 1861 a few pictures were taken at Kew, while from February 1862 to February1863 the instrument was in continuous operation at Mr. De la Rue's private observatory at Cranford, and from May 1863 until the present date it has been in continuous operation at Kew under Mr. De la Rue's superintendence. A Table was then given from which it was deduced that the number of groups observed at Kew from June to December 1863 inclusive was 64, while that observed by Hofrath Schwabe during the same interval was 69. In like manner, the number at Kew between January and November 1864 inclusive was 109, while during the same interval Hofrath Schwabe observed 126. It thus appears that Schwabe's numbers are somewhat larger than those of Kew; but probably, by means of a constant corrective, the one series may be made to dovetail with the other.

The authors then attempted to answer the following questions:—

(1) Is the umbra of a spot nearer the sun's centre than the penumbra, or, in other words, is it at a lower level?

(2) Is the photosphere of our luminary to be viewed as composed of heavy solid, or liquid matter, or is it of the nature either of a gas or cloud?

(3) Is a spot (including both umbra and penumbra) a phenomenon which takes place beneath thelevel of the sun's photosphereor above it?

In answering the first of these, it was shown that if the umbra is appreciably at a lower level than the penumbra, we are entitled to look for an apparent encroachment of the umbra upon the penumbra on that side which is nearest the visual centre of the disk. This, in fact, was the phenomenon which Wilson observed, and which led him to the belief that the umbra was nearer the sun's centre than the penumbra.

Two Tables are then given, showing the relative disposition of the umbra and penumbra for each spot of the Kew pictures available for this purpose.

In the first of these, this disposition was estimated from left to right, this being the direction in which spots advance across the visible disk by rotation; while in the second Table this disposition was estimated in a direction parallel to circles of solar longitude, and in this Table only spots having a high solar latitude were considered.

From the first of these Tables it was shown that, taking all those cases where an encroaching behaviour of the umbra in a right and left direction has been perceptible, 86 per cent. are in favour of the hypothesis that the umbra is nearer the centre than the penumbra, while 14 per cent. are against it. It also appeared that, taking *all available spots* and distributing them into zones according to their distance from the centre, this encroaching behaviour is greatest when spots are near the border, and least when they are near the centre.

From the second Table, in which only spots of high latitude were considered, it was shown that, taking all those cases where an encroaching behaviour of the umbra in an up-and-down direction has been perceptible, 80·9 per cent. are in favour of the hypothesis that the umbra is nearer the centre than the penumbra, while 19·1 per cent. are against it.

The result of these Tables is therefore favourable to this hypothesis.

The authors next endeavoured to answer the following question:—Is the photosphere of our luminary to be viewed as composed of heavy solid, or liquid matter, or is it of the nature either of a gas or cloud?

It was observed that the great relative brightness of faculæ near the limb leads to the belief that these masses exist at a high elevation in the solar atmosphere, thereby escaping a great part of the absorptive influence, which is particularly strong near the border; and this conclusion was confirmed by certain stereoscopic pictures produced by Mr. De la Rue, in which the faculæ appear greatly elevated. It was remarked that faculæ often retain the same appearance for several days, as if their matter were capable of remaining suspended for some time.

A Table was then given, showing the relative position of sun-spots and their accompanying faculæ for all the Kew pictures available for this purpose.

From this it appeared that out of 1137 cases 584 have their faculæ entirely or mostly on the left side, 508 have it nearly equal on both sides, while only 45 have it mostly to the right. It would thus appear as if the luminous matter being thrown up into a region of greater absolute velocity of rotation fell behind to the left; and we have thus reason to suppose that the faculous matter which accompanies a spot is abstracted from that very portion of the sun's surface which contains the spot, and which has in this manner been robbed of its luminosity.

Again, there are a good many cases in which a spot breaks up in the following manner. A bridge of luminous matter of the same apparent luminosity as the surrounding photosphere appears to cross over the umbra of a spot unaccompanied by any penumbra. There is good reason to think that this bridge is above the spot; for were the umbra an opaque cloud and the penumbra a semi-opaque cloud, both being above the sun's photosphere, it is unlikely that the spot would break up in such a manner that the observer should not perceive some penumbra accompanying the luminous bridge. Finally, detached portions of luminous matter sometimes appear to move across a spot without producing any permanent alteration.

From all this it was inferred that the luminous photosphere is not to be viewed as composed of heavy solid, or liquid matter, but is rather of the nature either of a gas or cloud, and also that a spot is a phenomenon existing below the level of the sun's photosphere.

The paper concluded with theoretical considerations more or less probable. Since the central or bottom part of a spot is much less luminous than the sun's photosphere, it may perhaps be concluded that the spot is of a lower temperature than the photosphere; and if it be supposed that all the sun's mass at this level is of a lower temperature than the photosphere, then we must conclude that the heat of our luminary is derived from without.

GEOLOGICAL SOCIETY.

[Continued from p. 159.]

December 21, 1864.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Coal-measures of New South Wales, with Spirifers, *Glossopteris*, and *Lepidodendron*." By W. Keene, Esq. Communicated by the Assistant-Secretary.

The prevailing rock in New South Wales is a sandstone, which is called the "Sydney Sandstone" by the author, and is the most recent deposit in the colony. Its upper beds contain certain shales, called the "False Coal-measures" by Mr. Keene, and the "Wyanamatta Beds" by the Rev. W. B. Clarke, the position of which is 800 feet above the true Upper Coal-seam. On approaching the latter, *Vertebraria australis* and *Glossopteris* are met with; and these

plants accompany the entire series of the Coal-measures, from the topmost to the lowest seam. The workable seams of coal were stated to be about eleven in number; and the author remarked that towards the two lowest seams *Pachydomus*, *Bellerophon*, &c. were found; *Spirifer* abounds near the lowest seam, as well as *Fenestella* and *Orthoceras*; but the *Vertebraria* and *Glossopteris*, occur throughout, while *Lepidodendron* has been found in coarse grits below the Coal-measures.

Mr. Keene then described a lower fossiliferous limestone unconformable to, and much older than, the Coal-measures; and gave a sketch of the geology of the Peak Downs Range, in Queensland. He concluded by referring to his large collection, sent to England some time ago, and now in the Bath Philosophical Institution, for further evidence of the age of the Coal-beds of New South Wales, which he believes to be as old as those of Europe.

2. "On the Drift of the East of England and its Divisions." By S. V. Wood, jun., Esq., F.G.S.

In this paper the author divides the Drift of the country extending from Flamborough Head to the Thames, and from the Sea on the East to Bedford and Watford on the West, as follows:—*a*, the Upper Drift, having a thickness of at least 160 feet still remaining in places. *b* and *c*, the Lower Drift, consisting of an Upper series (*b*), having a thickness from 40 to 70 feet, and a Lower series (*c*), with a thickness, on the coast near Cromer, of from 200 to 250 feet, but rapidly attenuating inland. *c* comprises the Boulder-till, and overlying contorted Drift of the Cromer coast, which along that line crop out from below *b* a few miles inland. *c* also, in an attenuated form, ranges inland as far south as Thetford, and probably to the centre of Suffolk, cropping out from below *b* by Dalling, Walsingham, and Weasenham, and appearing at the bottom of the valleys of central Norfolk. *b* consists of sands, which on the east coast overlie the Fluvio-marine and Red Crag, but change west and south into gravels, which pass under *a* and crop out again on the north, south, and centre of Norfolk, and west of Suffolk and Essex, extending (but capped in many places by *a*) over most of Herts. The Upper Drift (*a*) consists of the widespread Boulder-clay, which overlaps *b*, for a small space, on the south-east in Essex, and again at Horseheath, near Saffron Walden, but overlaps it altogether on the north-west, resting on the secondary rocks in Huntingdonshire and Lincolnshire. The distribution of *b* indicates it as the deposit of an irregular bay, afterwards submerged by the sea of *a*, which overspread a very wide area. *a* now remains only in detached tracts, having been extensively denuded on its emergence at the beginning of the post-glacial age, so that wide intervals of denudation (separating the tracts) indicate the post-glacial straits and seas which washed islands formed of *a*. The author considers the so-called Norwich Crag of the Cromer coast as *not* of the age of the Fluvio-marine Crag of Norwich, but as an arctic bed forming the base of *c*, into which it passes up uninterruptedly. The author regards the beds *b* as identical with the fluvio-marine gravels of Kelsea, near

Hull, and the Kelsea bed not to be above *a*, as hitherto supposed, but below it, having been forced up through *a* into its present position. He also regards the Upper Drift (*a*) as the equivalent of the Belgian Loess, and the beds *b* as the equivalent of the Belgian Sables de Campine.

ROYAL INSTITUTION OF GREAT BRITAIN.

Jan. 20, 1865. "On Combustion by Invisible Rays." By Professor Tyndall, F.R.S. &c.

Passing in review the researches and discoveries of the two Herschels, and the experiments of Melloni, Franz, and Müller on the dark rays of the sun, the lecturer came to the invisible radiation of the electric light, and to the distribution of heat in its spectrum. The instruments made use of were the electric lamp of Duboscq and the linear thermo-electric pile of Melloni. The spectrum was formed by means of lenses and prisms of pure rock-salt. It was equal in width to the length of the row of elements forming the pile; and the latter being caused to pass through its various colours in succession, and also to search the space right and left of the visible spectrum, the heat falling upon the pile, at every point of its march, was determined by the deflection of an extremely sensitive galvanometer.

As in the case of the solar spectrum, the heat was found to augment from the violet to the red, while in the dark space beyond the red it rose to a maximum. The position of the maximum was about as distant from the extreme red in the one direction, as the green of the spectrum in the opposite one.

The augmentation of temperature beyond the red in the spectrum of the electric light is sudden and enormous. Representing the thermal intensities by lines of proportional lengths, and erecting these lines as perpendiculars at the places to which they correspond, when we pass beyond the red these perpendiculars suddenly and greatly increase in length, reach a maximum, and then fall somewhat more suddenly on the opposite side of the maximum. When the ends of the perpendiculars are united, the curve beyond the red, representing the obscure radiation, rises in a steep and massive peak, which quite dwarfs by its magnitude the radiation of the luminous portion of the spectrum.

Interposing suitable substances in the path of the beam, this peak may be in part cut away. Water, in certain thicknesses, does this very effectually. The vapour of water would do the same; and this fact enables us to account for the difference between the distribution of heat in the solar and in the electric spectrum. The comparative height and steepness of the ultra-red peak, in the case of the electric light, are much greater than in the case of the sun, as shown by the diagram of Professor Müller. No doubt the reason is, that the eminence corresponding to the position of maximum heat in the solar spectrum has been cut down by the aqueous vapour of our atmosphere. Could a solar spectrum be produced beyond the limits of the atmosphere, it would probably show as steep a mountain of in-

visible rays as that exhibited by the electric light, which is practically uninfluenced by atmospheric absorption.

Having thus demonstrated that a powerful flux of dark rays accompanies the bright ones of the electric light, the question arises, Can we separate the one class of rays from the other?

In the lecturer's first experiments on the invisible radiation of the electric light, black glass was the substance made use of. The specimens, however, which he was able to obtain, destroyed, along with the visible, a considerable portion of the invisible radiation. But the discovery of the deportment of elementary gases directed his attention to other simple substances. He examined sulphur dissolved in bisulphide of carbon, and found it almost perfectly transparent to the invisible rays. He also examined the element bromine, and found that, notwithstanding its dark colour, it was eminently transparent to the ultra-red rays. Layers of this substance, for example, which entirely cut off the light of a brilliant gas-flame, transmitted its invisible radiant heat with freedom. Finally he tried a solution of iodine in bisulphide of carbon, and arrived at the extraordinary result that a quantity of dissolved iodine sufficiently opake to cut off the light of the midday sun was, within the limits of experiment, absolutely transparent to invisible radiant heat.

This, then, is the substance by which the invisible rays of the electric light may be almost perfectly detached from the visible ones. Concentrating by a small glass mirror, silvered in front, the rays emitted by the carbon-points of the electric lamp, we obtain a convergent cone of light. Interposing in the path of this concentrated beam a cell containing the opake solution of iodine, the light of the cone is utterly destroyed, while its invisible rays are scarcely, if at all, meddled with. These converge to a focus, at which, though nothing can be seen even in the darkest room, the following series of effects has been produced:—

A piece of black paper placed in the focus, is pierced by the invisible rays, as if a white-hot spear had been suddenly driven through it. The paper instantly blazes, without apparent contact with anything hot.

A piece of brown paper placed at the focus soon shows a red-hot, burning surface, extending over a considerable space of the paper, which finally bursts into flame.

The wood of a hat-box similarly placed is rapidly burnt through. A pile of wood and shavings, on which the focus falls, is quickly ignited, and thus a fire may be set burning by the invisible rays.

A cigar or a pipe is immediately lighted when placed at the focus of invisible rays. His Royal Highness the Comte de Paris performed this experiment at the lecture.

Disks of charred paper placed at the focus are raised to brilliant incandescence; charcoal is also ignited there.

A piece of charcoal, suspended in a glass receiver full of oxygen, is set on fire at the focus, burning with the splendour exhibited by this substance in an atmosphere of oxygen. The invisible rays, though they have passed through the receiver, still retain sufficient power to render the charcoal within it red-hot.

A mixture of oxygen and hydrogen is exploded in the dark focus, through the ignition of its envelope.

A strip of blackened zinc-foil placed at the focus is pierced and inflamed by the invisible rays. By gradually drawing the strip through the focus, it may be kept blazing with its characteristic purple light for a considerable time. This experiment is particularly beautiful.

Magnesium wire, flattened out and blackened, burns with almost intolerable brilliancy.

The effects thus far described are, in part, due to chemical action. The substances placed at the dark focus are oxidizable ones, which, when heated sufficiently, are attacked by the atmospheric oxygen, ordinary combustion being the result. But the experiments may be freed from this impurity. A thin plate of charcoal, placed *in vacuo*, is raised to incandescence at the focus of invisible rays. Chemical action is here entirely excluded. A thin plate of silver or copper, with its surface slightly tarnished by the sulphide of the metal, so as to diminish its reflective power, is raised to incandescence either *in vacuo* or in air. With sufficient battery-power and proper concentration, a plate of platinized platinum is rendered white-hot at the focus of invisible rays; and when the incandescent platinum is looked at through a prism, its light yields a complete and brilliant spectrum. In all these cases we have, in the first place, a perfectly invisible image of the coal-points formed by the mirror; and when the plate of metal or of charcoal is placed at the focus, the invisible image raises it to incandescence, and thus prints itself visibly upon the plate. On drawing the coal-points apart, or on causing them to approach each other, the thermograph of the points follows their motion. By cutting the plate of carbon along the boundary of the thermograph, we may obtain a second pair of coal-points, of the same shape as the original ones, but turned upside down; and thus by the rays of the one pair of coal-points, which are incompetent to excite vision, we may cause a second pair to emit all the rays of the spectrum.

The ultra-red radiation of the electric light is known to consist of æthereal undulations of greater length, and slower periods of recurrence, than those which excite vision. When, therefore, those long waves impinge upon a plate of platinum and raise it to incandescence, their period of vibration is changed. The waves emitted by the platinum are shorter, and of more rapid recurrence, than those falling upon it, the refrangibility being thereby raised, and the invisible rays rendered visible. Thirteen years ago Professor Stokes published the celebrated discovery that by the agency of sulphate of quinine, and various other substances, the ultra-violet rays of the spectrum could be rendered visible. These invisible rays of high refrangibility, impinging upon a proper medium, cause the molecules of that medium to oscillate in slower periods than those of the incident waves. In this case, therefore, the invisible rays are rendered visible by the *lowering* of their refrangibility; while in the experiments of the lecturer, the ultra-red rays are rendered visible by the *raising* of their refrangibility. To the phenomena brought to light by Professor Stokes, the term *fluorescence* has been applied by

their discoverer, and to the phenomena brought forward on Thursday week at the Royal Society, and on the evening of the following day at the Royal Institution, the lecturer proposes to apply the term *calorescence*.

It was the discovery, more than three years ago, of a substance opake to light, and almost perfectly transparent to radiant heat—a substance which cut the visible spectrum of the electric light sharply off at the extremity of the red, and left the ultra-red radiation almost untouched—that led the lecturer to the foregoing results. They lay directly in the path of his investigation, and it was only the diversion of his attention to subjects of more immediate interest, that prevented him from reaching, much earlier, the point which he has now attained. On this, however, the lecturer could found no claim, and the *idea* of rendering ultra-red rays visible, though arrived at independently, does not by right belong to him. The right to a scientific idea or discovery is secured by the act of publication; and, in virtue of such an act, priority of conception as regards the conversion of heat-rays into light-rays, belongs indisputably to Dr. Akin. At the Meeting of the British Association, assembled at Newcastle in 1863, he proposed three experiments, by which he intended to solve this question. He afterwards became associated with an accomplished man of science, Mr. Griffith, of Oxford, and jointly with him pursued the inquiry. Two out of the three experiments proposed at Newcastle are, the lecturer believed, impracticable; but the third, though not yet executed, may nevertheless be capable of execution. In that third Dr. Akin proposed to converge the rays of the sun by a concave mirror, to cut off the light by "proper absorbents," and to bring platinum-foil into the focus of invisible rays. It is quite possible that, had he possessed the instrumental means at the lecturer's disposal, or had he been sustained as the lecturer had been, both by the Royal Society and the Royal Institution, Dr. Akin might have been the first to effect the conversion of the dark heat-rays into luminous ones. For many years the idea of forming an intense focus of invisible rays had been perfectly clear before the lecturer's mind, and in 1862 he published experiments upon the subject. He had then discovered the properties of iodine, and had made use of this substance as an absorbent, in the manner subsequently proposed by Dr. Akin. The effects observed by him in 1862 at the focus of invisible rays, were such as no previous experimenter had witnessed, and no experimenter could have observed them without being driven to the results which formed the subject of the evening's discourse. Still publication is the sole test of scientific priority, and it cannot be denied that Dr. Akin was the first to propose definitely to change the refrangibility of the ultra-red rays of the spectrum, by causing them to raise platinum-foil to incandescence.

XXXV. *Intelligence and Miscellaneous Articles.*

ON THE HEATING OF THE GLASS PLATE OF THE LEYDEN JAR BY THE DISCHARGE. BY DR. WERNER SIEMENS.

AS it seemed probable to me that the glass plate of the Leyden jar must be heated by the charge and discharge, I arranged an ap-

paratus by which even small heating-effects could be recognized with certainty. The result of the experiments made therewith quite answered my expectations. The construction of the apparatus is as follows:—I covered with silk a fine iron and equally fine German-silver wire. These wires were cut into pieces about a decimetre in length, and each German-silver wire soldered to an iron wire. These wires were so laid upon a glass plate covered with a cement of resin and shell-lac, that the solderings of 180 wires, without touching, occupied a space of about a square decimetre. By pressing with a warm iron the wires were fused into the cement and thus fastened upon the plate. After the adjacent free ends of the wires were soldered together so as to form a battery of 180 elements, a second glass plate, also covered with cement, was laid with the cemented surface upon the first. By careful heating, the cement between the glass plates was softened, and a portion of it, with the individual air-bubbles which it enclosed, pressed out. The thermo-pile stood thus in a surface of cement free from air, exactly in the middle of a glass plate about 5 millims. in thickness. The middle of the glass plates covering thus all the inside solderings was provided on both sides with tinfoil armatures about a decimetre square, which were furnished with insulated wires. The free ends of the thermo-pile were also furnished with copper wires, by which they were connected with a delicate reflecting galvanometer. The entire apparatus, including the external solderings, was carefully protected from any change of temperature. A short succession of charges and discharges, by means of a voltaic inductor of about an inch striking-distance, was sufficient to drive the scale of my galvanometer out of the field, and this, too, in the direction due to the heating of the solderings between the armatures. After the charges the deflection returns very slowly to zero. It disappears entirely only after some hours. It is independent of the direction of the discharge, and apparently proportional to the number of charges, and to the striking-distance to which the apparatus was charged. The motion of the scale begins at once, and then proceeds regularly. But if one of the armatures be touched with the finger, the scale remains stationary two or three seconds before beginning its motion, which usually terminates outside the field of view.

The heating-effect observed can be due neither to conduction through the mass of glass, nor to compression by attraction of the armatures, nor, finally, to penetration of electricity into the glass-mass nearest the armatures. The first objection is directly answered by the arrangement of the apparatus and the experiments described. Any heating by compression would be equalized by the subsequently equally strong cooling on expansion, and could therefore produce no permanent heating, even if the extremely small contraction were sufficient. Nor could a penetration of electricity into the mass of glass nearest the coatings be the cause of the heating, as the deflection did not begin at once, but only after the lapse of some seconds. But if we assume, with Mr. Faraday, that the charge and discharge depends on a progressive molecular motion in the insulator which separates the coatings, the fact of the heating of this insulator is no longer surprising.—*Berliner Berichte*, 1864, p. 612.

A LETTER FROM SIR J. F. W. HERSCHEL, BART., TO THE EDITORS OF THE PHILOSOPHICAL MAGAZINE AND JOURNAL, IN REFERENCE TO A RECENT COMMUNICATION TO THAT WORK OF DR. J. DAVY, ETC.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN, Collingwood, February 12, 1865.

In reference to the communication from Dr. Davy in your Journal for this month, I have only a few words to say. If the conversation between Dr. Wollaston and Sir Humphry Davy, the purport of which I stated in my letter to you of December 20, 1864, did not take place, I must have been dreaming when I wrote down, within forty-eight hours from the time of its occurrence, the statement in question. If so, the dream has been a singularly persistent one, as I still retain a clear recollection of that conversation having taken place, and of its general purport, though not of the particular words used; and I have, moreover, before me the copy of a letter which I wrote to Mr. Davies Gilbert on the 27th of June, 1827, enclosing my formal resignation of the Secretaryship, in which the opinion expressed by Dr. Wollaston on that occasion respecting the President's right or courtesy of nomination is alluded to. This letter I subjoin; observing merely that, as Mr. Gilbert was present at the Meeting of the 23rd of the previous November, the allusion to a matter of which he was personally cognizant requires no further explanation. (Copy.)

"Devonshire Street, June 27, 1827.

"DEAR SIR,—I have sent a copy of the enclosed to each member of the Council, the President excepted, you acting as his delegate. which I hope will satisfy all points of propriety.

"I should retire from the office of Secretary, I will confess, with more regret had it not been recently pressed on my attention with somewhat of a painful distinctness, that that office, by the usages of the Royal Society, although elective by a majority of its members, is yet regarded as being held at the nomination of the individual filling the office of the President; and had it not even been advanced by an authority, to which on all occasions I feel disposed to bow with just reverence, that the exercise of such power of nomination is and ought to be regarded as an act of patronage on the President's part.

"When I accepted the office of Secretary, I assuredly regarded it in no such light, considering myself honoured by the election of the Society to an office so important to its interests, and which had been dignified by the tenure of a Hooke and a Wollaston. Although I never will subscribe to a doctrine which, by lowering the dignity of that office in the eyes of those who may hereafter fill it, cannot but tend to abate their zeal in the discharge of its duties by leading them to regard as a matter of mere routine and clerkship what ought to be executed in the spirit of an important scientific trust—yet I cannot but most deeply lament that such an opinion should exist:—and offer it as my individual and humble, but fixed, senti-

timent that unless very distinctly disavowed in all future cases, the interests of the Royal Society and of science will suffer materially.

"I remain, dear Sir, very sincerely yours,

(Signed) "J. F. W. Herschel."

"*Davies Gilbert, Esq., V.P.R.S., 45 Bridge Street, Westminster.*"

It can hardly be necessary for me to recall to Dr. Davy's recollection that I have nowhere asserted that the President had promised the Secretaryship to Mr. Babbage, or either directly or by implication promised to appoint him to, or to give him, that office.

I have the honour to be, Gentlemen,

Your obedient Servant,

J. F. W. Herschel.

LUNAR INFLUENCE OVER TEMPERATURE.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

With great deference and respect for Dr. Buys Ballot, I cannot but express a very strong conviction that the value of results derived from a tabulation of mean temperatures of the different days of the moon's age for the last seventy-five years (from 1789 to 1865) far outweighs any that attaches to the sixty years previous.

In an inquiry into lunar influence over temperature, the most delicate and accurate instruments are essential in order to detect effects which may often require to be measured by tenths and hundredths of a degree (Fahr.). No thermometer, however, of great accuracy existed between 1723 and 1789; and even if any had been available, it would seem extremely doubtful whether observations 130 years ago were made with sufficient regularity and care, or at the exact hours requisite to obtain true mean results. In Dr. Buys Ballot's work, *Les changements périodiques de température*, p. 22, there is a passage which of itself would suggest a doubt on this point.

It is remarkable, too, that the existence of cold after full moon first becomes apparent in the sums of temperature at Harlem shortly after the date of the invention of Six's self-registering spirit-thermometer.

My results (which also show cold in the second half of the lunation) completely accord with conclusions arrived at by Bouvard, Schübler, Flaugergues, and other physicists, who have sought for lunar influence over rain, cloud, the serenity of the sky, &c. I have consequently from the first attributed the occurrence of heat and cold in the lunation to the presence or absence of cloud as a known cause of the phenomenon. There is indeed no meteorological fact more certain than that the greatest cold occurs under a clear sky, whether in the polar regions or the desert of Sahara, and that heat is retained in the ground and lower strata of the air by the agency of cloud*.

As regards the want of perfect regularity in the action of the moon, Dr. Buys Ballot's suggestion that it may be due to her influence being partly direct and partly indirect, is worth careful consi-

* As shown by Glaisher, in the Phil. Trans.

deration. But it is consistent with my theory to attribute it also to the fact that the existence of cloud and vapour is due to the sun and the winds, and consequently the action of the moon, though consistent, cannot be regular or constant.

For example, if no clouds are formed at the period of new moon and first quarter, the earth's radiant heat passes away into space, and the result is *cold* at a time when curves of mean temperature for a series of lunations, show *heat*; and so in like manner, if more heat than the moon is able to disperse occurs in the second half of the lunation, where the curves indicate cold, the result, *pro hac vice*, would be a higher temperature than the average.

Lastly, as the moon's hemisphere turned towards the earth does not, as it would appear, attain its maximum heat until the last quarter (and it is probable also that it does not part with all its radiant heat till several days after new moon), if any further experiments are made with the thermo-electric pile, would it not be well to try for heat at the third quarter, and that in a lunation in which the moon takes the longest time in passing from the full, and so receives the greatest amount of heat from exposure to the sun's rays?

Your faithful Servant,

Ewhurst, February 14, 1865. J. Park Harrison.

P.S.—The cloud-dispelling power of the moon has been observed not only by Sir John Herschel, but by Baron Humboldt, M. Le Verrier, the late Radcliffe observer (at Oxford), and Mr. Nasmyth; I am not aware if anyone but Sir John Herschel has attributed it to the radiant heat of our satellite.

ELECTRICAL STANDARD.

To the Editors of the Philosophical Magazine and Journal.

6 Duke Street, Adelphi, London, W.C.,

Gentlemen, February 7, 1865.

I have the honour to inform you that copies of the Standard of Electrical Resistance chosen by the Committee on Electrical Standards appointed by the British Association in 1861, can now be procured by application to me as Secretary to the Committee.

A unit coil and box will be sent on the remittance of £2 10*s*.

The Standard is a close approximation to 10,000,000 $\frac{\text{metre}}{\text{seconds}}$ in Weber's absolute electromagnetic system, determined according to new and careful experiments made by different members of the Committee, and the copies are constructed of an alloy of platinum and silver in a form chosen as well adapted for exact measurement.

The want of a generally recognized standard of electrical resistance has been universally felt, and led to the appointment of the Committee. They now desire me to express a hope that the motives which have led to their present choice, and which are fully explained in the several reports published by them, will induce you to assist in procuring the general adoption of the new standard,

I remain, Gentlemen,

Your obedient Servant,

(Signed) Fleeming Jenkin,

Secretary to the Committee.

THE

LONDON, EDINBURGH, AND DUBLIN

PHILOSOPHICAL MAGAZINE

AND

JOURNAL OF SCIENCE.

[FOURTH SERIES.]

APRIL 1865.

XXXVI. *On the Application of the Principle of the Screw to the Floats of Paddle-wheels.* By W. G. Adams, *M.A., F.G.S., Fellow of St. John's College, Cambridge, Lecturer on Natural Philosophy at King's College, London**.

THE following calculation is an attempt to apply the mathematical laws of fluid-motion, as far as they may be depended on, to an important practical question with regard to paddle-wheel steamers, viz. the comparison of the pressure of a fluid on a float in the form of the surface of a screw, with the pressure on the ordinary flat float, so as to discover whether any advantage would be gained by having the floats of paddle-wheels made in the form of a screw-surface. This application of the principle of the screw has been conceived by Dr. Croft of St. John's Wood, who has tested it by means of small working models with very satisfactory results, both as to the *speed* when the boat is either *lightly* or *heavily* laden, and also as to the *steadiness* of the pulling power. In all cases on the small boat, the new is superior to the old paddle-wheel when they are of the same diameter, and even when the new wheel is narrower than the old one. It has also been tried on larger boats, and the two kinds of floats have been compared; and the results have been in favour of the new principle, there being no large waves or strong backward current in the case of the new wheel, and the velocity and steadiness of motion being greater than in the paddle-wheel with flat floats. This principle is now being applied, under Dr. Croft's directions, to one of Her Majesty's steamers.

Suppose A B (fig. 2) to be the axis of the paddle-wheel, and A G

* Communicated by the Author.

and B H two parallel radii on opposite rims of one wheel, C E F D the axis of two screw surfaces C E K G and D F L H of opposite kinds, the outer edges G K and H L being at the circumference of the wheel, and the angles which they make with H G at G and H being each half a right angle; C E, E F, and F D are equal to one another, so that between the floats a vacant space is left equal in breadth to either of them. K L is a rod connecting the free corners of the floats, and the planes K E M and L F N are perpendicular to the axis. These two floats take the place of one of the ordinary flat floats; and I propose to compare the pressure on them, and the power necessary to sustain it for a given speed of revolution with the pressure, and the corresponding power, in the case of the ordinary paddle-wheel. Before entering into the calculation, it will be well to notice some of the peculiar features of this paddle-wheel, which will probably prove to be not among the least of its advantages. If it is possible to judge from the conduct of a small model, they certainly appear to be decided advantages.

1. The floats enter the water *gradually*, the points K and L being first immersed, and the whole gradually following; so that there is no sudden jerking of the steamer when a float enters the water, as in the case of the ordinary wheel, where the jerking is so violent at starting as to cause vibrations up and down by the bending of the ship from stem to stern. The absence of this jerking and the quivering motion will be no slight comfort to those on board.

2. These floats do not carry all the water before them, or cause a strong backward current and so diminish the pressure on the succeeding floats as in the ordinary wheel, but, as there is a vacant space between the floats equal in breadth to one-third of the breadth of the wheel, the water in this space is only slightly disturbed, and this disturbance is not sufficient to cause any very great motion at the point where the next float enters the water; also since the water runs off the floats both outwards and inwards, and not in the direction of motion, there will be no large waves formed in the wake of the ship; so that the power which is expended in the ordinary wheel in producing large waves is in this case usefully expended in increasing the speed of the ship; this is fully borne out in the case of Dr. Croft's working models; also the water thrown under the ship on both sides will help to buoy it up.

In these points it would seem that Dr. Croft's wheel is likely to be superior to the ordinary wheel, whether the floats of the latter are feathering floats or not.

3. In the new wheel, when the float is coming out of the water, the water runs off sideways readily, and is not carried up with

the float, as it must be when the only way for it to run off is in the direction of the motion of the float; hence by this method all the back-water is got rid of; and this is perhaps one of the greatest advantages which this wheel has over the ordinary wheel without feathering floats.

4. Again, the new wheel will work whether it be only slightly immersed or whether the water rises nearly up to the axis of the wheel; and it seems to have the greatest advantage over the ordinary wheel when they are both deeply laden.

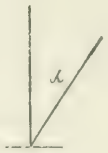
There are other important features which might be pointed out; but these I have no doubt Dr. Croft will take an opportunity of bringing before the notice of the scientific world; but I have mentioned these few points to show that it is not unlikely that this invention may lead to our more steady and more rapid onward progress by means of paddle-wheel steamers.

Two questions still remain: Can sufficient hold on the water be obtained by means of the new floats? and if so, what power will be necessary to obtain it? To these questions it is proposed to give a mathematical solution, based on the laws of resistance on surfaces in motion through fluids, which laws, although they are not sufficiently perfect to give *absolute* results in any particular case, may yet be employed to *compare* the pressures on two surfaces under similar conditions, and so in this case may be expected to show whether the new wheel is any improvement on the old, either in getting up the same speed with less power, or in getting up a greater speed with engines of the same horse-power.

Fig. 3.



Fig. 1.

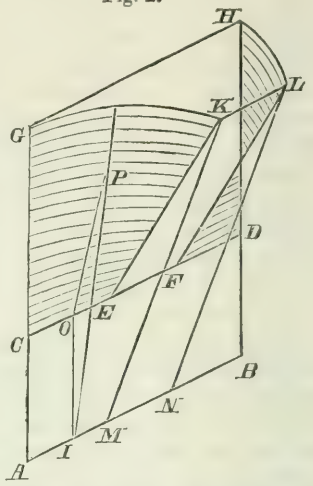


By the mathematical theory, the resistance perpendicular to the surface on any part of a surface in motion through a fluid, where the direction of the motion makes an angle α with the perpendicular to the surface, is $\frac{1}{2}\kappa\rho_1 v^2 \cos^2\alpha$, where ρ_1 is the

density of the fluid, κ the area, and v the velocity of that portion of the surface. The effect of this resistance in a direction inclined at an angle β to the normal, will be measured by $\frac{1}{2}\kappa\rho_1 v^2 \cos^2\alpha \cos\beta$.

Suppose A B G H to have made half a revolution round A B from left to right, so that the floats are in their lowest position, then the direction of motion of *any point of the surface* being perpendicular to the line drawn from it perpendicular to the axis, we must find the *angle between* the direction of motion and the perpendicular to the surface, and also between the perpendicular to the surface at that point, and the perpendicular to the plane A B H G, since the motion of *the vessel* is perpendicular to this plane. If α be the former angle and β the latter, then the resistance $R = \frac{1}{2}\kappa\rho_1 v^2 \times \cos^2\alpha \cos\beta$. Also the power of engine required will be measured by that part of the resistance perpendicular to the surface ($\frac{1}{2}\kappa\rho_1 v^2 \cos^2\alpha$), which acts in a plane perpendicular to the axis A B, multiplied by its distance from that axis—in other words, the moment of the resistance about the axis of revolution.

Fig. 2.



To express these inclinations and portions of surface mathematically, we must have recourse to the ordinary notation of Geometry of Three Dimensions.

The surface of this screw is traced out by a straight rod, as O P, which slides uniformly along a fixed rod, as C D, at right angles to it, and at the same time twists uniformly about that fixed rod. It will be familiar to all as the surface formed by the edges of the stairs in a spiral staircase, and the steepness of the stairs will depend on the relation between the sliding and the twisting motion, and also on the distance from the shaft.

If the fixed rod C D be taken as the axis of z, and C G as the axis of x, then the equation to the surface C G E K is

$$x \sin\frac{z}{r} - y\cos\frac{z}{r} = 0,$$

where, in this particular case, C G $= r$, since the angle between G K and H G at G is 45°. If O P, the distance of any point P from the axis of the surface $= \rho$, then $\rho^2 = x^2 + y^2$, and the

angle which O P makes with the plane G C D H (the plane of $x\,z$) $=\frac{z}{r}$, the surface being a conoid, of which the helix is the directrix, and which is inclined at an angle of 45° to the plane of xy, at a distance r from the axis. Hence

$$x=\rho\cos\frac{z}{r}\text{ and }y=\rho\sin\frac{z}{r}.$$

The inclination of the normal at any point to the axis of z is

$$\cos^{-1}\frac{\left(x\cos\frac{z}{r}+y\sin\frac{z}{r}\right)}{\sqrt{r^2+\left(x\cos\frac{z}{r}+y\sin\frac{z}{r}\right)^2}},$$

which is equal to $\cos^{-1}\frac{\rho}{\sqrt{r^2+\rho^2}}=\gamma$, suppose.

Hence an element of the surface at the point P is

$$\frac{\sqrt{r^2+\rho^2}}{\rho}\,dx\,dy.$$

The inclination of the normal to its trace on the plane of $x\,y$ is

$$\cos^{-1}\frac{r}{\sqrt{r^2+\rho^2}}.$$

This trace is perpendicular to O P in the plane P O I (fig. 3), and therefore makes an angle O P I with the direction of motion of the point P.

Let A C $=$ O I $=a$, and let the inclination of O P $\left(\frac{z}{r}\right)$ to the plane of $x\,z$ be denoted by θ, and let the angle P I O $=\phi$; then the angle O P I $=\theta-\phi$

$$\cos(\theta-\phi)=\frac{x(a+x)+y^2}{\sqrt{x^2+y^2}\sqrt{(a+x)^2+y^2}}=\frac{\rho+a\cos\theta}{\sqrt{(a+x)^2+y^2}}.$$

α being the inclination of the normal to the direction of motion of P, we get

$$\cos\alpha=\sin\gamma\cos(\theta-\phi)=\frac{r}{\sqrt{r^2+\rho^2}}\times\frac{\rho+a\cos\theta}{\sqrt{(a+x)^2+y^2}}.$$

Also, β being the inclination of the normal to the axis of y (the direction of the required resistance),

$$\cos\beta=\sin\gamma\cos\theta=\frac{r\cos\theta}{\sqrt{r^2+\rho^2}}.$$

Also, if ω be the angular velocity of the wheel, then the velocity of the point P $=\omega\times$ I P $=\omega\times\sqrt{(a+x)^2+y^2}$.

Hence $$R = \tfrac{1}{2}\rho_l v^2 \kappa \cos^2\alpha \cos\beta$$

$$= \tfrac{1}{2}\rho_l \omega^2 \{(a+x)^2 + y^2\} \left(\frac{\sqrt{r^2+\rho^2}}{\rho} dx\, dy\right) \frac{r^2}{r^2+\rho^2} \frac{(\rho + a\cos\theta)^2 . r\cos\theta}{\{(a+x)^2+y^2\} . \sqrt{r^2+\rho^2}}$$

$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \frac{(\rho + a\cos\theta)^2 \cos\theta}{\rho(r^2+\rho^2)} dx\, dy;$$

or, taking polar coordinates,

$$R = \tfrac{1}{2}\rho_l \omega^2 r^3 . \frac{(\rho + a\cos\theta)^2 \cos\theta}{r^2+\rho^2} d\rho\, d\theta.$$

Again, the perpendicular distance from the axis A B on the trace of the normal in the plane of xy is

$$\rho + a\cos\theta.$$

Therefore M, the moment about the axis A B

$$= \tfrac{1}{2}\rho_l v^2 \kappa \cos^2\alpha \sin\gamma (\rho + a\cos\theta)$$

$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \frac{(\rho + a\cos\theta)^3}{r^2+\rho^2} d\rho\, d\theta.$$

Each float extends to a distance $(a+r)$ from the axis A B, *i. e.* to the circumference of the wheel. Therefore for the limits of ρ

$$a^2 + \rho^2 + 2a\rho\cos\theta = (a+r)^2,$$

or

$$\rho + a\cos\theta = \sqrt{r^2 + 2ar + a^2\cos^2\theta}.$$

The limits for ρ are

$$\sqrt{r^2 + 2ar + a^2\cos^2\theta} - a\cos\theta \text{ and } 0.$$

The limits of integration for θ will be one-third of the ratio of the breadth of the wheel to the length of C G.

In the example worked out, C G has been taken $= \frac{2}{3}$ A G, and the breadth of the wheel G H has been taken slightly greater than C G for convenience; so that the limits for θ are *rather more than* $\frac{1}{3}$ or 20°, and 0, and the breadth of the wheel is to its radius as 7 : 10.

The expressions for the resistance and moment about the axis admit of integration with respect to ρ without any restriction:

$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \iint \frac{(\rho + a\cos\theta)^2 \cos\theta}{r^2+\rho^2} d\rho\, d\theta$$

$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \iint \frac{(\rho^2 + r^2)\cos\theta + 2a\rho\cos^2\theta + (a^2\cos^2\theta - r^2)\cos\theta}{r^2+\rho^2} d\rho\, d\theta$$

$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \left\{ \iint \cos\theta\, d\rho\, d\theta + \iint a\cos^2\theta . \frac{2\rho}{r^2+\rho^2} d\rho\, d\theta \right.$$

$$\left. + \iint \frac{(a^2\cos^2\theta - r^2)\cos\theta}{r^2+\rho^2} d\rho\, d\theta \right\}$$

$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \left\{ \int \rho \cos\theta \, d\theta + \int a \cos^2\theta \log\left(1+\frac{\rho^2}{r^2}\right) d\theta \right.$$
$$\left. + \int \frac{a^2\cos^2\theta - r^2}{r} \cos\theta \,.\, \tan^{-1}\frac{\rho}{r} \cdot d\theta \right\}$$

between proper limits,

$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \left\{ \int \{ \sqrt{r^2+2ar+a^2\cos^2\theta} - a\cos\theta \} \cos\theta \, d\theta \right.$$
$$+ \int a\cos^2\theta \log\left\{ 1+\left(\frac{\sqrt{r^2+2ar+a^2\cos^2\theta}-a\cos\theta}{r} \right)^2 \right\} d\theta$$
$$\left. - \int \frac{r^2-a^2\cos^2\theta}{r} \cdot \cos\theta \,.\, \tan^{-1}\left(\frac{\sqrt{r^2+2ar+a^2\cos^2\theta}-a\cos\theta}{r} \right) d\theta \right\}.$$

The moment of the resistance on one float

$$M = \tfrac{1}{2}\rho_l \omega^2 r^3 \iint \frac{(\rho + a\cos\theta)^3}{r^2+\rho^2} \, d\rho \, d\theta$$
$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \left\{ \iint (\rho \, d\rho \, d\theta + 3a\cos\theta \, d\rho \, d\theta) \right.$$
$$+ \iint \frac{(3a^2\cos^2\theta - r^2)}{r^2+\rho^2} \rho \, d\rho \, d\theta$$
$$\left. - \iint \frac{3ar^2\cos\theta - a^3\cos^3\theta}{r^2+\rho^2} \, d\rho \, d\theta \right\}$$

between proper limits,

$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \left\{ \int (\tfrac{1}{2}\rho^2 + 3a\rho\cos\theta) \, d\theta \right.$$
$$+ \int \frac{(3a^2\cos^2\theta - r^2)}{2} \log\left\{ 1+\frac{\rho^2}{r^2} \right\} d\theta$$
$$\left. - \int \frac{3ar^2\cos\theta - a^3\cos^3\theta}{r} \tan^{-1}\frac{\rho}{r} \cdot d\theta \right\}$$
$$= \tfrac{1}{2}\rho_l \omega^2 r^3 \left[\int \left\{ \frac{\sqrt{r^2+2ar+a^2\cos^2\theta} + 5a\cos\theta}{2} \right\} \right.$$
$$\left\{ \sqrt{r^2+2ar+a^2\cos^2\theta} - a\cos\theta \right\} d\theta$$
$$\left. + \int \frac{3a^2\cos^2\theta - r^2}{2} \log\left\{ 1+\left\{ \left(\frac{\sqrt{r^2+2ar+a^2\cos^2\theta}-a\cos\theta}{r} \right) \right\}^2 \right\} d\theta \right.$$

$$-\int \frac{3ar^2\cos\theta - a^3\cos^3\theta}{r}\tan^{-1}\frac{\sqrt{r^2+2ar+a^2\cos^2\theta}-a\cos\theta}{r}\,d\theta\Big].$$

Now let $a=\frac{r}{2}$.

Then

$$\mathrm{R}=\tfrac{1}{2}\rho_l\omega^2r^4\Big\{\int\Big(\sqrt{1+1+\frac{\cos^2\theta}{4}}-\frac{\cos\theta}{2}\Big)\cos\theta\,d\theta$$
$$+\int\frac{\cos^2\theta}{2}\cdot\log\Big\{1+\Big(\sqrt{2+\frac{\cos^2\theta}{4}}-\frac{\cos\theta}{2}\Big)^2\Big\}d\theta$$
$$-\int\frac{4-\cos^2\theta}{4}\,.\cos\theta\,.\tan^{-1}\Big(\sqrt{2+\frac{\cos^2\theta}{4}}-\frac{\cos\theta}{2}\Big)\,d\theta\Big\}.$$

The resistance on the two floats will be equal to one another. Therefore the resistance on a pair of floats

$$=\tfrac{1}{2}\rho_l\omega^2r^4\Big\{\int\big(\sqrt{8+\cos^2\theta}-\cos\theta\big)\cos\theta\,d\theta$$
$$+\int\log\Big(1+\Big(\frac{\sqrt{8+\cos^2\theta}-\cos\theta}{2}\Big)^2\Big)\cos^2\theta\,d\theta$$
$$-\int\tfrac{1}{2}(4-\cos^2\theta)\cos\theta\tan^{-1}\Big(\frac{\sqrt{8+\cos^2\theta}-\cos\theta}{2}\Big)\,d\theta\Big\}.$$

Also the moment of resistance on a pair of floats

$$=\tfrac{1}{2}\rho_l\omega^2r^5\Big\{\int\Big(2+\frac{\cos^2\theta}{4}-\frac{5\cos^2\theta}{4}+2\cos\theta\sqrt{2+\frac{\cos^2\theta}{4}}\Big)\,d\theta$$
$$-\int(1-\tfrac{3}{4}\cos^2\theta)\log\Big\{1+\Big(\frac{\sqrt{8+\cos^2\theta}-\cos\theta}{2}\Big)^2\Big\}\,d\theta$$
$$-\int\cos\theta(3-\tfrac{1}{4}\cos^2\theta)\tan^{-1}\Big(\frac{\sqrt{8+\cos^2\theta}-\cos\theta}{2}\Big)\,d\theta\Big\}$$

$$=\tfrac{1}{2}\rho_l\omega^2r^5\Big\{\int\{2+\cos\theta(\sqrt{8+\cos^2\theta}-\cos\theta)\}d\theta$$
$$-\int\log\Big\{1+\Big(\frac{\sqrt{8+\cos^2\theta}-\cos\theta}{2}\Big)^2\Big\}\,d\theta$$
$$+\int\tfrac{3}{4}\cos^2\theta\log\Big\{1+\Big(\frac{\sqrt{8+\cos^2\theta}-\cos\theta}{2}\Big)^2\Big\}\,d\theta$$
$$-\int2\cos\theta\tan^{-1}\Big(\frac{\sqrt{8+\cos^2\theta}-\cos\theta}{2}\Big)d\theta$$

$$-\int \tfrac{1}{4}(4-\cos^2\theta)\cos\theta\tan^{-1}\left(\frac{\sqrt{8+\cos^2\theta}-\cos\theta}{2}\right)d\theta\Bigg\}.$$

To find the value of these expressions for the resistance and moment, the method of Integration by Quadratures has been employed, the values of the functions to be integrated being found at intervals of 5° between the limits 0 and 20°, which limits make the ratio of the breadth to the radius of the wheel the same as that of 7 to 10.

The value of the circular measure of 5° is ·087266.

Values of the Functions to be integrated, the Integrations being taken between 0° *and* 20°.

Let

$$(\sqrt{8+\cos^2\theta}-\cos\theta)=2q.$$

	$\cos\theta$.	$\cos^2\theta$.	$2q$.	$2q\cos\theta$.
− 2½	·999048	·998098	2·000635	1·998731
+ 2½	·999048	·998098	2·000635	1·998731
7½	·991445	·982963	2·005714	1·988555
12½	·976296	·953154	2·015886	1·968101
17½	·953717	·909576	2·031174	1·937165
22½	·923879	·853553	2·051613	1·895442

	$\log_e(1+q^2)$.	$\log_e(1+q^2)\cdot\cos^2\theta$.	$\tan^{-1}q\cdot\cos\theta$.	$\tan^{-1}q\cdot\cos\theta\frac{4-\cos^2\theta}{2}$.
− 2½	·693465	·692146	·784869	1·177960
+ 2½	·693465	·692146	·784869	1·177960
7½	·696004	·684146	·780093	1·176785
12½	·701090	·668250	·770643	1·174015
17½	·708733	·644647	·756422	1·168832
22½	·718950	·613662	·737407	1·160105

Hence the expressions for the resistance with their differences are:—

− 2½°	1·512917		
		·000000	
+ 2½	1·512917	−·017001	−·017001
7½	1·495916	−·033579	−·016578
12½	1·462337	−·049357	−·015778
17½	1·412980	−·063980	−·014623
22½	1·349000		

The expressions for the moment are:—

− 2½	1·665778		
		·000000	
+ 2½	1·665778	−·008696	−·008696
7½	1·657082	−·017176	−·008480
12½	1·639906	−·025249	−·008073
17½	1·614657	−·032784	−·007535
22½	1·581873		

To find the accurate values of the integrals of these functions, suppose α is one of the angles for which the value is known, then we must integrate for the variable angle θ between the limits $+2\frac{1}{2}^\circ$ and $-2\frac{1}{2}^\circ$ on each side of α, and then add the results of the integrations.

Let
$$\theta = cx + \alpha,$$
where c is the circular measure of 5°, so that the limits for x are $+\frac{1}{2}$ and $-\frac{1}{2}$.

If $f_I(0)$ and $f_{II}(0)$ be the mean of the first differences and the second difference corresponding to one of the values of α, then
$$f(x) = f(0) + xf_I(0) + \frac{x^2}{2} f_{II}(0);$$
therefore
$$\int f(x)dx = xf(0) + \frac{x^2}{2} f_I(0) + \frac{x^3}{6} f_{II}(0),$$
and
$$\int_{-\frac{1}{2}}^{\frac{1}{2}} f(x) \,.\, c \,.\, dx = c\{f(0) + {}_{2}^{1}{}_{4} f_{II}(0)\}.$$

The sum of the second differences (which is the same as the last of the first differences) divided by 24,

$= -\cdot 002666$ for the resistance, and

$= -\cdot 001366$ for the moment.

Hence the correction for the resistance
$$= -\cdot 002666 \,.\, c,$$
and the correction for the moment
$$= -\cdot 001366 \,.\, c;$$
therefore the resistance becomes
$$\begin{aligned} &\tfrac{1}{2}\rho_l \omega^2 r^4 \,.\, c\{5\cdot 884150 - \cdot 002666\} \\ &\quad = \tfrac{1}{2}\rho_l \omega^2 r^4 \times c(5\cdot 881484) \\ &\quad = \tfrac{1}{2}\rho_l \omega^2 r^4 \times \cdot 513256 = \tfrac{1}{2}\rho\omega^2(\text{rad})^4 \times \cdot 101384, \end{aligned}$$
and the moment becomes
$$\begin{aligned} &\tfrac{1}{2}\rho_l \omega^2 r^5 c\{6\cdot 577423 - \cdot 001366\} \\ &\quad = \tfrac{1}{2}\rho_l \omega^2 r^5 \,.\, c \times (6\cdot 576057) \\ &\quad = \tfrac{1}{2}\rho_l \omega^2 r^5 \times \cdot 573869 = \tfrac{1}{2}\rho_l \omega^2 (\text{rad})^5 \times \cdot 075572, \end{aligned}$$
the resistance and moment being expressed in powers of the radius of the *wheel* for the sake of comparison with the resistance and moment on a flat float.

Now considering the resistance and moment on a flat float where the radius of the wheel and the length and breadth of the float are in the ratio of 10 : 6 : 3 respectively. In this case the resistance on one float

$$= \tfrac{1}{2}\rho_1\omega_1^2 \cdot (\tfrac{6}{10}r) \cdot \int_{\frac{7}{10}r}^{r} \rho^2 d\rho$$

$$= \tfrac{1}{2}\rho_1\omega_1^2 \times \tfrac{6}{10}r \times \tfrac{1}{3}r^3\{1-(\tfrac{7}{10})^3\}$$

$$= \tfrac{1}{2}\rho_1\omega_1^2 \times r^4 \times \cdot 1314.$$

The moment

$$= \tfrac{1}{2}\rho_1\omega_1^2 r^5 \cdot \tfrac{6}{10} \times \tfrac{1}{4}\{1-(\tfrac{7}{10})^4\}$$

$$= \tfrac{1}{2}\rho_1\omega_1^2 r^5 \times \cdot 113985.$$

On comparing these results, we see that when the two wheels are revolving with the same angular velocity, the resistance and moment on the new float are less than on the old, but the ratio between the work done and the power required to perform it is *greater* in the case of the *new* wheel than in that of the *old*, in the ratio of 7 to 6; so that the advantage is on the side of Dr. Croft's invention. If the difference of the powers of the two be spent in increasing the angular velocity of the new wheel, then the resistance will be greater in the new wheel than in the old, in the ratio of 7 to 6; and the angular velocities in this case would be in the ratio of 6 to 5.

The mathematical theory takes no account of the motion of the water; so that the results of the above calculations are quite separate from the other evident advantages stated in the early part of this paper. Also, since the floats enter the water *gradually*, and since the water is less disturbed, it is plain that the angular velocity of the new wheel may be increased in the above ratio without causing the floats to slip through the water and do no work.

Another case, where the results may be more easily obtained from the expressions after the first integration, is that in which the axis of the wheel is the axis of the screw-surface, and where the inner portion of the surface up to a distance of one-third of the radius from the axis is cut out, so that $a=0$, and the limits of integration for ρ are r and $\frac{r}{3}$. Hence

$$R = \tfrac{1}{2}\rho_1\omega^2 r^4\left\{\int_0^{\theta}(2-\tfrac{2}{3})\cos\theta\, d\theta - 2\int_0^{\theta}\left(\frac{\pi}{4}-\tan^{-1}\tfrac{1}{3}\right)\cos\theta\, d\theta\right\}$$

$$= \tfrac{1}{2}\rho_1\omega^2 r^4 \times \sin\theta \times \cdot 406,$$

and

$$M = \tfrac{1}{2}\rho_1\omega^2 r^5\left\{\int_0^{\theta}\{1-(\tfrac{1}{3})^2\}\, d\theta - \int_0^{\theta}\log_e(\tfrac{2}{3})d\theta = \tfrac{1}{2}\rho_1\omega^2 r^5 \times \theta \times \cdot 301\right.$$

very nearly.

Hence if these wheels be of the same breadth as in the first case, the resistance and moment are not so great as in the other case, but are nearly in the same proportion. If the breadth of the wheel be three-fourths of the radius, then the same resistance will be produced by the same power as in the first case considered.

It will now not be difficult to arrive at the expressions for the resistances and moments, taking into account the velocity of the steamer. If v be this velocity, the expression for the resistance when the float is in a vertical position is

$$\iint \tfrac{1}{2}\rho_l \cdot \frac{\sqrt{r^2+\rho^2}\times r^3}{(r^2+\rho^2)^{\frac{3}{2}}} \cos\theta \,.\, \{\omega(\rho + a\cos\theta) - v\cos\theta\}^2 d\rho \,.\, d\theta$$

$$= \tfrac{1}{2}\rho_l \iint \frac{r^3\cos\theta}{r^2+\rho^2} \{\omega(\rho+a\cos\theta) - v\cos\theta\}^2 d\rho\, d\theta.$$

Also the moment

$$= \tfrac{1}{2}\rho_l r^3 \iint \frac{\rho + a\cos\theta}{r^2+\rho^2} \{\omega(\rho+a\cos\theta) - v\cos\theta\}^2 d\rho\, d\theta.$$

Hence the resistance on one float becomes

$$\tfrac{1}{2}\rho_l \omega^2 r^3 \iint \frac{(\rho+a\cos\theta)^2\cos\theta}{r^2+\rho^2} d\rho\, d\theta$$
$$- \tfrac{1}{2}\rho_l r^3 v\omega \iint \frac{2\rho\cos^2\theta}{r^2+\rho^2} d\rho\, d\theta$$
$$- \tfrac{1}{2}\rho_l r^3 v(2\omega a - v) \iint \frac{\cos^3\theta}{r^2+\rho^2} d\rho\, d\theta.$$

And the moment on one float becomes

$$\tfrac{1}{2}\rho_l \omega^2 r^3 \iint \frac{(\rho+a\cos\theta)^3}{r^2+\rho^2} d\rho\, d\theta$$
$$- \rho_l r^3 \,.\, v\omega \iint \frac{(\rho+a\cos\theta)^2\cos\theta}{r^2+\rho^2} d\rho\, d\theta$$
$$+ \tfrac{1}{4}\rho_l r^3 v^2 \iint \frac{2\rho\cos^2\theta}{r^2+\rho^2} d\rho\, d\theta$$
$$+ \tfrac{1}{2}\rho_l r^3 v^2 \,.\, a \,.\, \iint \frac{\cos^3\theta}{r^2+\rho^2} d\rho\, d\theta.$$

These integrals are the same as those which have already been found; and the results arrived at are these:—

Resistance on a pair of floats

$$=\tfrac{1}{2}\rho_l\omega^2r^4\left\{\cdot513256-\frac{2v}{\omega r}\times\cdot234563-\frac{2v}{\omega r}\times\frac{\omega r-v}{\omega r}\times\cdot259202\right\};$$

and the moment of the resistance on them

$$=\tfrac{1}{2}\rho_l\omega^2r^5\left\{\cdot573869-\frac{2v}{\omega r}\times\cdot513256+\frac{v^2}{\omega^2r^2}\times\cdot493765\right\}.$$

The angular velocity should be such that v, the rate of the steamer, is not greater than $\frac{\omega r}{2}$. Let $v=\frac{\omega r}{2}$:

then the resistance

$$=\frac{1}{2}\rho_l\omega^2r^4\times\cdot149092$$

$$=\frac{1}{2}\rho_l\omega^2\,(\text{rad})^4\times\cdot029450;$$

and the moment

$$=\frac{1}{2}\rho_l\omega^2r^5\times\cdot184054$$

$$=\frac{1}{2}\rho_l\omega^2\,(\text{rad})^5\times\cdot024238.$$

The corresponding expressions may be obtained for the flat float by taking $(\omega\rho-v)^2$ for the square of the actual velocity of a point of the surface, instead of $\omega^2\rho^2$; and the results obtained are:—

Resistance

$$=\frac{1}{2}\rho_l\omega_l^2r^4\left\{\cdot1314-\frac{2v}{\omega_lr}\times\cdot153+\frac{v^2}{\omega_l^2r^2}\times\cdot18\right\}$$

$$=\frac{1}{2}\rho_l\omega_l^2r^4\times\left\{\cdot1314-\frac{2\omega}{\omega_l}\times\cdot051+\frac{\omega^2}{\omega_l^2}\times\cdot02\right\};$$

Moment

$$=\frac{1}{2}\rho_l\omega_l^2r^5\left\{\cdot1140-\cdot1314\times\frac{2v}{\omega_lr}+\frac{v^2}{\omega_l^2r^2}\times\cdot153\right\}$$

$$=\frac{1}{2}\rho_l\omega_l^2r^5\left\{\cdot1140-\frac{2\omega}{\omega_l}\times\cdot0438+\frac{\omega^2}{\omega_l^2}\times\cdot017\right\},$$

the velocities being the same in both cases. If the relation between the angular velocities be such that the resistances are the same, then the moment in the case of the new wheel will be less than in the old, the ratios being as 96·88 to 100; so that not only can the same hold be obtained on the water by means of the new floats, and so the speed be kept up, but the power of

the engine required to do it is about 3 per cent. less. The angular velocity of the new wheel will be greater than that of the old in the ratio of 1·175 to 1, or nearly in the ratio of 6 to 5. We may conclude, then, that for a velocity of ten miles an hour in a steamer with paddle-wheels of 15 feet radius, the number of revolutions a minute should not be less than 18: and with this number the relations between the resistances and moments are those given above, which show an advantage in favour of the new wheel. With a less number of revolutions the part of a float nearest to the axis will tend to stop rather than to propel the boat. For a velocity of 18 miles an hour, the number of revolutions a minute in the new wheel should not be less than 30, for the same reason; and then the same hold is obtained on the water as in the ordinary wheel making 25 revolutions a minute, and that with less power of engine. By still further increasing the angular velocity in the new wheel as compared with the old, so that the number of revolutions a minute are as 3 to 2, the ratio of the resistance to the power producing it is 10 per cent. greater in the new wheel than in the old. Since it appears that the angular velocity of the new wheel may be made much greater than that of the old (provided marine engines will admit of such an increase in the rate of revolution) without causing the floats to slip through the water, it follows that this increase will give the new wheel a great advantage over the old with regard to speed.

XXXVII. *On Chemical Nomenclature, and chiefly on the use of the word Acid. By* G. C. Foster, *B.A., Lecturer on Natural Philosophy in Anderson's University, Glasgow*.*

IN forming a nomenclature for any science, two distinct requirements must be kept in view as having each to be supplied. In the first place, a convenient general language must be provided, to serve as a medium for the ordinary every-day transactions of the science; and in the second place, there must be what may be called the legal language of the science, —a language whose terms are, as far as possible, strictly defined, and have an exact and generally recognized value. It is this second stricter language which constitutes the more technical part of scientific nomenclature, and it is this alone which it is either desirable or possible to alter or reform in accordance with any particular state of scientific opinion. The general language of a science will always, in the main, take care of itself; and at any given period it usually contains a large admixture of terms— once technical, but now no longer used for purposes of accu-

* Communicated by the Author.

racy—which, like fossils in a rock, tell of the successive changes by which the existing state of things has been brought about. The more strictly technical language, on the other hand, is always formed with more or less premeditation, and is therefore, to a corresponding extent, under control and capable of being reformed. The existence of such a language and its preservation in the highest possible state of efficiency are of the utmost scientific importance; for, although none but a pedant would in all cases employ it (when the use of more popular expressions could lead to no ambiguity), it is quite certain that accurate language is an essential instrument of accurate thought, and that the progress of any science will be greatly retarded unless its language is such as to admit of its facts and theories being stated with any required degree of precision.

These general remarks are meant as answer, by anticipation, to the objection, so often urged to attempted reforms in Chemical Nomenclature, that the proposed modes of expression would be too troublesome for general use. It is contended that this objection, even when real, is by no means necessarily conclusive. A particular innovation might render important services as a part of what has above been called the legal language of the science, although it might never come into general use, from the fact that it corresponded to a degree of precision beyond that required for ordinary purposes. Our aim should be to render our language such that it may be in the power of every chemist to know accurately the ideas which he will convey to the minds of others by his use of it. The extent to which each one avails himself of its resources must, of course, be left to his own discretion.

At the present time, one of the most important questions which has to be considered, in relation to chemical nomenclature, is as to the use of the term Acid. The sense in which this word has been employed of late years has been gradually becoming more uniform than it was previously; and those chemists who regard the now prevalent usage as the correct one might well have been content to watch, without attempting to hasten, its still further spread, were it not that a return to a former and, as they consider, improper use of the term has quite recently found an advocate of no less authority than Professor Williamson, President of the Chemical Society*.

This distinguished chemist desires to apply the term acid to the class of substances now very frequently called *anhydrides* or *anhydrous acids*, and to call, for instance, N^2O^5 nitric acid, N^2O^3 nitrous acid, $(C^2H^3O)^2O$ acetic acid, SO^2 sulphurous

* "Remarks on Chemical Nomenclature and Notation," by Professor Williamson: Journ. Chem. Soc. vol. xvii. p. 421 (December 1864).

acid, SO^3 sulphuric acid, &c. Among the arguments urged by Professor Williamson in support of this proposal, one of the most forcible is founded upon what he says was the original meaning of the word. "There are perhaps no words in use among chemists of which the original meaning was so clear as the word acid, and the correlative word base. They were introduced to describe bodies of opposite properties, which are more or less completely lost in the salt or compound of acid and base" (page 423). And again, "I submit that it is not allowable to use any word in a sense inconsistent with its established meaning and use, unless one has the most ample evidence that it cannot again be wanted, and will not again be used, for its original pupose. Words may be considered as the property of the ideas which they are used to denote, and the words 'acid' and 'base' belong to the idea of compounds of fundamentally opposite properties, which unite to form one or more molecules of a comparatively neutral compound" (page 424).

No doubt Dr. Williamson's definition of the words acid and base would have met with very general, though not universal, acceptance fifty years ago; but if we wish to ascertain the "original" meaning of these words, we must go much further back, and then that meaning becomes anything but particularly clear. There can be no doubt that *sourness* was the property whose possession in common by different substances first led to the use of the term *acid* as a generic name for a class of bodies. Corrosiveness came afterwards to be regarded as an important mark of acidity; but at no time previous to Lavoisier does it appear that the meaning of the word acid was defined with anything like precision. In addition to sourness and corrosiveness, the power of reddening many vegetable blues, of precipitating sulphur from solution of liver of sulphur, of causing effervescence with alkaline carbonates, and of destroying more or less completely the causticity of alkalies, was considered characteristic of acidity; but how far chemists were from using the word acid to denote *any* very clearly definable idea, is sufficiently proved by the fact that, in the latter half of the last century, limestone was supposed to become caustic when burned by absorbing an acid (*acidum pingue*) from the fire, and that its power of rendering the "mild alkalies" caustic was due to its imparting to them this same acid*.

Until the time of Lavoisier, the substances known as acids were regarded as members of the general class of *salts*, which was commonly divided into *salia acida*, *salia alcalina*, and *salia*

* The *acidum pingue* of Meyer was the last form of an idea which had been previously put forth by Van Helmont and by Stahl. (See Watts's 'Dictionary of Chemistry,' vol. i. pp. 40 & 115.)

media vel *composita*. The separation of acids from neutral and alkaline salts, and their recognition as a distinct class, resulted chiefly from Lavoisier's discovery of the presence of oxygen in several of the best-known acids, and his consequent conclusion that they were in reality a particular class of oxides*. This view was of course applicable with strictness only to the anhydrous acids, but it did not for a good while occasion any difficulty in the application of the word acid: the essential nature of the difference between the bodies since known as anhydrides, and the bodies formed by their union with water, although insisted upon by Davy, was not generally recognized until experiment had shown how different was the behaviour of these two sets of substances respectively with ammonia and almost all organic compounds. But these investigations had been pursued only a very little way before it became evident, to all who took the trouble to examine the question, that it was utterly illogical and unphilosophical to continue to designate by the same name bodies which differed so essentially from each other as did many of those which had been hitherto classed indiscriminately as acids. Laurent and Gerhardt, whose teaching contributed more than that of any other chemists to set this point in a clear light, got rid of the inconsistency by strictly confining the application of the name *acid* to salts of hydrogen, such as SO^4H^2, and calling the bodies (actually or conceivably) produced from these by loss of water, such as SO^3, *anhydrides*. In retaining the term *acid* for SO^4H^2 and its analogues, rather than for SO^3 and its analogues, they undoubtedly followed the prevailing usage of chemical language; for though the latter class of bodies were described as acids in all systematic works on chemistry, yet in perhaps ninety-nine cases out of a hundred, when an acid was spoken of as taking part in or resulting from a reaction, it was a *hydrogen-salt*, and not an anhydrous acid, that was meant. Hence the course which they adopted obviously involved a much smaller departure from established usage than the choice of the other alternative would have done; and if this was true twenty years ago, it is true to so much greater an extent now that the reversal of Laurent and Gerhardt's system would necessitate the rewriting of almost the whole of organic chemistry.

Hence it appears that, so far from the original meaning of the word acid having been clear, it would be more correct to say that this word was never used in a strictly scientific and logical sense at all, until Gerhardt defined acids to be *salts whose base was wholly composed of hydrogen*†.

* See particularly his *Traité élémentaire de Chimie*, vol. i. p. 163.

† *Précis de Chimie Organique* (Paris, 1844), vol. i. p. 70; *Introduction à l'étude de la Chimie par le Système Unitaire* (Paris, 1848), p. 103.

The original meaning of the term *base* is perhaps even less clear than that of the word acid. According to Kopp* it was introduced by French authors, and occurs frequently from about the year 1730. With regard to its earliest meaning, he says, "The following passage from Rouelle's 'Memoir on the Neutral Salts' (1754) may give some idea of the sense connected with the use of this word :—'J'ai étendu le nombre des sels autant qu'il était possible, en définissant génériquement le sel neutre, un sel formé par l'union d'un acide avec une substance quelconque, qui lui sert de base et lui donne une forme concrète ou solide.' (Stahl employs a periphrasis similar to the above to express what we now call a base; in his *Specimen Becherianum* [1702], he denotes the substance which is contained in chloride of sodium in combination with an acid as *materia illa, quæ sali corpus præbet.*)" The word "base" occurs very frequently in the writings of Lavoisier, but with him, as in the above quotation from Rouelle, it appears to have retained very much of its etymological meaning of *foundation*, and not in any degree to suggest the possession of properties opposed to those of acids. The following passage may serve to illustrate his use of the term :—"Il est infiniment rare d'y trouver [in the vegetable kingdom] un acide simple, c'est-à-dire qui ne soit composé que d'une seule base acidifiable. Tous les acides de ce règne ont pour base l'hydrogène et le carbone, quelquefois l'hydrogène, le carbone et le phosphore, le tout combiné avec une proportion plus ou moins considérable d'oxygène. Le règne végétal a également des oxides qui sont formés des mêmes bases doubles et triples, mais moins oxygénées" (*Traité élémentaire*, vol. i. pp. 124, 125, edit. 1789). The modern word equivalent to 'base' in this passage would evidently be 'radical,' as is clearly shown by the one which follows :—"Il faut donc distinguer dans tout acide, la base acidifiable à laquelle M. de Morveau a donné le nom de radical, et le principe acidifiant, c'est-à-dire, l'oxigène" (Ibid. p. 69).

The limitation of the term base to *salifiable bases* only, seems to be of comparatively recent introduction, and is probably due to Berzelius; but even with this limitation it has never been confined exclusively to bodies of any one class. Ever since the expression salifiable base came into use, it is probable that almost all chemists have been agreed in considering it as applicable to substances as variously constituted as those represented by the formulæ KHO, K^2O, PbO, NH^3. In the following passage, however, Professor Williamson seems to imply that Gerhardt confined the word base to *basic hydrates* only :—"In fact he [Gerhardt] systematically applied the term acid to hydrogen-salts, giving

* *Geschichte der Chemie*, vol. iii. p. 69.

the name anhydride to acids, and leaving bases, however anhydrous they might be, entirely unprovided with a corresponding name" (page 424); but I am not aware from what part of his writings such a limitation can be inferred. It is quite true, however, that Laurent and Gerhardt pointed out that, as a matter of fact, the bases which took part in a great many reactions were not anhydrous oxides (as they had hitherto been commonly represented) but hydrates, and that they laid great stress upon the importance of recognizing this *fact* in the chemical equations by which those reactions were expressed.

But not only can I not admit that original usage is in favour of that application of the terms "acid" and "base" which Dr. Williamson recommends; it appears to me that one at least of the arguments by which he tries to show that the salts of hydrogen ought not to be called acids, can be used with equal force against the employment of that name in the sense which he defends. In page 426 he says,—

"But it is not only true that the bodies misnamed anhydrides are acids; it is equally true and certain that the hydrogen-salts cannot with any consistency be called acids: for when two hydrogen-salts—say hydric nitrate and potassic hydrate—react on one another, we cannot call the process a combination of nitric acid with potash, without putting in the background, and to some extent concealing, the fact that water is formed, quite as much as potassic nitrate. Learners of chemistry who have been told that an acid is a thing which combines with a base, naturally and consistently wish to omit any mention of the water in their description of the process, and they have to be told that this supposed acid really is a salt of hydrogen possessing acid properties, and the so-called base is a hydrogen-salt with strongly basic properties, the two on coming together undergoing double decomposition, just as truly as potassic chloride when mixed with argentic nitrate."

Without dwelling upon the facts that chemists who call hydrogen-salts acids, *do not want* to call the process above described "a combination of nitric acid with potash," and that they would not tell learners of chemistry that "an acid is a thing which combines with a base," we may remark that a precisely similar difficulty would in some cases arise for learners from the adoption of Dr. Williamson's definition of acids. When acetic "acid," $(C^2H^3O)^2O$, aceto-benzoic "acid," $\left.\begin{matrix}C^2H^3O\\C^7H^5O\end{matrix}\right\}O$, or thiacetic "acid," $(C^2H^3O)^2S$, reacts with a base, say water, H^2O (which is the first base in the list of examples on page 429), we cannot call the process a combination of the acid with the base, without putting in the background, and to some extent

concealing, the fact that it is in reality a double decomposition, just as truly as the change which occurs on mixing potassic chloride with argentic nitrate.

Possibly Professor Williamson might reply to this, that he never asserted that double decompositions could not occur between acids and bases; but that is not now the precise question. If the impossibility of correctly describing the reaction which takes place between the two bodies NHO^3 and HKO as the combination of an acid with a base is a reason for not calling NHO^3 nitric acid, the like impossibility in the case of the bodies $(C^2H^3O)^2O$ and H^2O is an equally good reason for not calling $(C^2H^3O)^2O$ acetic acid.

Nevertheless, although it appears to me that the only consistent and logical sense in which the word acid can be used, is the sense defined by Laurent and Gerhardt, it seems to me unnecessary to retain it *at all* as a strictly scientific term. I most fully agree with what Professor Williamson says in a passage which follows immediately the one last quoted:—

"I hold that it is inconsistent and highly inconvenient to apply to the double decompositions which take place between hydrogen-salts of acid properties and hydrogen-salts of basic properties, any terms which conceal the fact of their close analogy with other double decompositions; and that the hydrogen-salts ought to be designated by terms similar in form and general arrangement to the terms applied to the salts of other metals."

If we regard the salts of hydrogen as constituted like the salts of any other metal, the application to them of the name *acid* becomes incorrect if it implies any peculiarity of constitution, and superfluous if it does not. When we want to speak of acids as a class, they are accurately and conveniently indicated as *hydrogen-salts*; while individual acids may equally well be denoted by such names as *hydric sulphate, hydric nitrate, hydric chloride,* &c., the systematic adoption of which is urged by Dr. Williamson.

The word acid will certainly long remain as a part of popular, and even of ordinary chemical language, and hence the importance of trying to ascertain its correct application; but its strictly scientific significance has passed away. It indicates a distinction to which we now know that no real difference corresponds.

Lastly, it is necessary to say a few words as to the nomenclature of the substances upon which Dr. Williamson is wishful to bestow the name acid. The term "anhydride," by which Laurent and Gerhardt designated these bodies, has been very justly objected to on the ground that, in applying it to any object, we merely state that that object is *not* one of an infinite number of

things which it might be, but we do not say what it *is*. This, again, is a term by the total abandonment of which I venture to think that the language of chemistry would be improved rather than impoverished. The so-called anhydrides are exclusively oxides (or sulphides); and there seems to be no good reason for applying to them a nomenclature different from that which is employed for other bodies of the same class. By simply calling these bodies what everyone admits that they are, namely, OXIDES, we avoid all the objections that can be urged against calling them either acids or anhydrides, and we obtain names—such as *sulphurous oxide, sulphuric oxide, acetic oxide, benzoic oxide*—which are at once intelligible to every chemist.

In support of this suggestion, I may be allowed to quote in conclusion part of a letter received a short time ago from Mr. Watts, a gentleman whom all recognize as a high authority on such a subject:—

"I quite agree with you that *oxide* is a much better term than *anhydride* for things like SO^3, P^2O^5, &c. Indeed I should have denoted them in that way throughout the 'Dictionary,' had it not been that some vested interests seemed to stand in the way. I allude to terms like *carbonic oxide, nitric oxide*, &c., which, being already appropriated, could not be applied to the anhydrides CO^2 and N^2O^5. But the difficulty may be completely got over by calling CO carbonic oxide, CO^2 carbonic dioxide, N^2O^5 nitric pentoxide, &c. As to the term 'acid,' I really don't see why it should not, as you suggest, be superannuated altogether, excepting as a trivial name for certain well-known compounds which people in general will perhaps never be induced to call by any other name."

XXXVIII. *On the Mechanical Energy of Chemical Actions.* By Dr. H. W. SCHRÖDER VAN DER KOLK*.

IN a communication on "Dissociation," M. H. Sainte-Claire Deville sets out from the view that all chemical compounds are ultimately decomposable into their constituents by a sufficiently elevated temperature†. He supposes the molecules thus

* Translated from Poggendorff's *Annalen*, vol. cxxii. p. 439 (July 1864), by G. C. Foster, B.A. The author says, in a foot-note to the original, "This paper was already written when I again encountered the same ideas in Clausius's recently published paper (Pogg. *Ann.* vol. cxxi. p. 1), where he says that in the principle of Transformations a general natural tendency to transformations in one definite direction expresses itself, and that this tendency comes into account also in the changes of state of material bodies."

† *Fortschritte der Physik*, 1860, p. 379. Phil. Mag. S. 4. vol. xx. p. 448.

separated to be, at a lower temperature, capable either of combining spontaneously (that is, by mere cooling) or of remaining separate. From this point of view, therefore, compounds may be divided into two groups,—the first containing those which, when decomposed by heat, are recomposed again when cooled; and the second, those in which this does not take place.

There seems to me to be a connexion between this property and the following. It is known from Favre and Silbermann's experiments that in many cases of chemical combination heat is evolved, while in a few cases heat is absorbed; accordingly, chemical compounds may be divided into two series from this point of view also. The way in which this property is connected with that previously referred to, will become clear from the following considerations.

If we figure to our minds a body, at first in some definite condition, at 0° C. for example, and then suppose heat to be applied to it, it will at a given temperature have taken up a given quantity of heat. This heat goes partly to raise its temperature, partly to cause molecular changes (so-called internal work), and partly to perform external work. This whole quantity of heat increases continually as the temperature rises. Elevation of temperature, as well as change of state of aggregation from the solid to the liquid, and from the liquid to the gaseous state, always requires absorption of heat. In order to find the quantity of heat existing in the body after the process, we must deduct from the total quantity the portion which is converted into external work. The quantity of heat then remaining is called by Thomson* the mechanical energy of the body in the given state. According to the suppositions we have made, this is not an absolute measure of the energy, but indicates how much more energy is accumulated in the body than was in it in a given condition at 0° C. Kirchhoff† has called the influence which the body exerts upon external matter, while passing from the first condition to the second, the effective function (*Wirkungsfunction*) of the body for this change. This is therefore, with the contrary sign, the exact equivalent of what Thomson calls its energy.

According to this, every body possesses, in a given condition, a given quantity of energy. Let us now suppose two bodies, such as oxygen and hydrogen, which can combine by an electric spark. Before combination, each contains a certain quantity of energy; by the combination heat is generated; and if the vapour of water which is formed is cooled down to the temperature existing before the combination, the quantity of energy which the

* Phil. Mag. S. 4. vol. ix. p. 523.

† Poggendorff's *Annalen*, vol. ciii. p. 177.

water-vapour contains less than was contained in its constituents will be exactly equal to the amount of heat set free. It will be understood that the combination of oxygen and hydrogen is here supposed to take place in a closed space, and therefore without any development of external work; otherwise this must also be taken into account.

If aqueous vapour is decomposed at the same temperature, as much energy must be given to it as was set free by the combination.

Accordingly, two cases are possible: either the body possesses more energy than its constituents, or it contains less.

In the first case, heat is produced by the decomposition of the body; in the second case it is destroyed.

Heat is therefore set free when we heat a compound of the first class until decomposition begins. The components will then no longer recombine spontaneously by subsequent cooling, for the quantity of energy contained in them is not sufficient for the formation of the compound body at the same temperature. Combination would be possible only if one of two cases occurred: either the body must take heat from surrounding matter, whose temperature never exceeds, but is at most equal to its own; or there must be a sudden cooling of the body formed. So far as I know, such a case of cooling has not hitherto presented itself.

The law developed above may accordingly be enunciated thus:

Bodies which evolve heat when decomposed by elevation of temperature are not reproduced by subsequent cooling.

Hence follows, as a direct consequence, the connexion of the above-named properties. Let us test this law by means of Favre and Silbermann's results*.

1. Nitrous oxide evolves heat when decomposed. There exists therefore less energy in nitrogen and oxygen when they are separate, than when they are combined in the form of this compound; accordingly they do not recombine when afterwards cooled.

This conclusion is not affected by Favre and Silbermann's explanation that ozone comes into play here.

2. Binoxide of hydrogen evolves heat on decomposition; and hence again there exists more energy in H^2O^2, than in H^2O and O when they are separate. By cooling, the combination is not reproduced.

3. Oxide of silver appears likewise to give off heat when decomposed; and it is not formed again by cooling.

4. According to Favre†, hypochlorous and chloric acids evolve

* *Annales de Chimie et de Physique*, 3 sér. vol. xxxvi. p. 1.

† *Thèses presentées à la Faculté des Sciences de Paris* (Mallet-Bachelier, 1853). In this work (p. 52), Favre has already remarked that

heat when decomposed; they break up when heated, and are not reproduced on cooling.

The following additional examples may be quoted from Deville's paper referred to above—namely, chloride, iodide, and sulphide of nitrogen, which are suddenly decomposed with evolution of heat by elevation of temperature, but are not reproduced by subsequent cooling. The law above stated holds good also for the transformations of dimorphous or polymorphous substances from one condition to another.

1. When Arragonite is heated, it changes into calc-spar and evolves heat; Arragonite therefore contains more energy than calc-spar, and hence the opposite change does not take place on cooling.

2. Crystals of sulphur obtained by fusion (and belonging to the Fifth System) change at a low temperature, with evolution of heat, into crystals of the same form as those of native sulphur (which belong to the Fourth System). Again, therefore, more energy is contained in the substance in its first condition than in the second, and accordingly the inverse process does not occur at low temperatures.

3. Plastic sulphur (*soufre mou*) likewise evolves heat while passing into ordinary sulphur, and hence contains more energy than this. Ordinary sulphur therefore cannot of itself change into the plastic modification, but the opposite change can occur.

4. Deville* speaks of a third modification of sulphur, which is insoluble in sulphide of carbon. By warming, this variety is converted into common sulphur; and it follows from the experiments of Fordos and Gelis†, that it takes up heat during the change, thus exhibiting the opposite behaviour to that of plastic sulphur. This insoluble modification accordingly contains less energy than ordinary sulphur, and apparently it does not change spontaneously into the latter. Deville‡ kept some fragments at the ordinary temperature, which were unchanged after six years.

Herein lies the explanation of the following observations by Favre§. He finds that in the formation of sulphurous acid from ordinary sulphur, and its further oxidation to sulphuric acid by means of chlorine, each equivalent evolves 67,212 thermal units (the equivalent of hydrogen being taken as 1 gramme), whereas by direct oxidation by means of hypochlorous acid only 64,110 thermal units are evolved. In the last case, however, the less

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the above-named compounds do not form of themselves after decomposition; but he merely deduces this from the observed phenomena, and does not give it as in any way a consequence of a general theory.

* *Ann. de Chim. et de Phys.* 3 sér. vol. xlvii. p. 94.

† Ibid. p. 108. ‡ Ibid. p. 100.

§ *Thèses*, &c., p. 43.

energetic insoluble modification was employed; and since in this experiment the components contained less energy than in the former one, but the resulting sulphuric acid the same quantity in both cases, the difference between the energy of the reagents and that of the products, that is, the heat evolved, was less in the second case.

5. It is pointed out in the same memoir (Favre, *Thèses, &c.*, p. 25) that 1331 units of heat are absorbed in the conversion of opake arsenious acid into the vitreous modification. Hence more energy exists in the second modification than in the first; and in reality vitreous arsenious acid changes spontaneously into the opake variety, but the opposite change takes place only on heating.

6. Red phosphorus possesses more energy than common yellow phosphorus*; nevertheless it does not of itself change into the latter. This, however, does not at all contradict the theorem; for this does not say that the transformation will occur in every case, but only that, if transformation does take place, the body passes into a state in which it contains less energy.

7. Ozone also may be quoted as an example. It contains more energy than ordinary oxygen, into which it is changed by heating, no doubt evolving heat at the same time. Subsequent cooling does not reproduce the ozone.

In accordance with the foregoing facts, it may be established as a general theorem, that—

When a body on heating passes from one condition to another with evolution of heat, it does not return to its first condition upon subsequent cooling.

The examples already given may suffice to illustrate this theorem. It is possible that exceptions will come to light; but if so, they must be such as can be referred to the two cases stated above. It is of course understood that the chemical affinity between molecules must also be taken into account, and we can imagine this sufficiently powerful to draw from surrounding objects the energy required for the combination. Something of

* [There appears to be a mistake here: according to Favre's experiments, as reported in Liebig and Kopp's *Jahresbericht* for 1853, p. 24, the transformation of 31 grammes of red phosphorus (1 gramme-atom) into common phosphorus is attended with the *absorption* of 28,246 thermal units. This result, taken in connexion with the comparative ease with which yellow phosphorus changes into the red modification, harmonizes better with the principles which the author is seeking to establish, than his own statement contained in the text. In Miller's 'Elements of Chemistry' (3rd ed. vol. ii. p. 220), however, it is stated that when red phosphorus is heated to the temperature at which it is converted into yellow phosphorus, "the whole mass suddenly passes back into the ordinary form, with a copious evolution of heat."—Transl.]

this kind takes place in freezing-mixtures, where the cooling is a secondary consequence of the powerful molecular forces; but similar phenomena do not seem to have been observed in the case of simple combination accompanied by absorption of heat.

The converse theorem to the above is as follows:—

If heat is absorbed in the case of a decomposition caused by elevation of temperature, an action of the opposite kind will occur on subsequent cooling.

This theorem cannot be referred to a theoretical basis, and therefore cannot be looked upon as proved; it receives, however, frequent confirmation, as the following examples show.

Carbonate of lime absorbs heat, according to Favre and Silbermann, when decomposed by heat, and therefore does not contain as much energy as its components taken together. Accordingly, lime and carbonic acid combine again on cooling.

It is worthy of remark that Arragonite, which contains more energy, absorbs scarcely any heat when decomposed.

It is the same with the slaking of lime. A great deal of heat is given off, and consequently hydrate of lime contains much less energy than its components. By heating, it is decomposed, and it re-forms spontaneously on cooling.

Heat is evolved in the formation of carbonic acid and water; consequently these compounds possess less energy than their constituents. Their elements, however, unite only at a high temperature; and if therefore, as stated by Deville, they are decomposed by the mere action of heat, this decomposition must take place at a still higher temperature than that at which they are formed. At low temperatures, nevertheless, no combination occurs, notwithstanding that the components contain much more energy than the products. This is connected with the mode of action of chemical affinity, which at low temperatures is not sufficiently powerful to cause the formation of these compounds, although they contain so much less energy. In order that a compound may be produced, two general conditions must be fulfilled: (1) there must be *chemical force* or *affinity* sufficient for combination; and (2) there must be the *energy* necessary for combination. There is to a certain extent an analogy between these conditions and those involved in the existence of a galvanic current, which requires not only a difference of tension, but, in addition, the energy needed for the production of the current, and generated by the chemical processes which go on in the battery. One cause alone is in both cases insufficient. Here therefore we may speak of reversible and non-reversible processes. If a body is changed by elevation of temperature and evolves heat, it comes into a new condition in which it contains less energy than before, and therefore cannot possibly return of

itself to its previous condition of more energy. This is accordingly a non-reversible process. If, on the other hand, heat is taken up during the change, the body in its new condition possesses more energy than before, and hence it may come to pass that it returns spontaneously on cooling to its former state.

The first case will be of most frequent occurrence, for here the second condition is of necessity always fulfilled. Hence it appears that evolution of heat in combination is the rule, and absorption of heat the exception.

As examples of reversible processes, we may also mention the phenomena of latent heat in fusion and vaporization. Bodies which have undergone these changes always possess more energy in their new condition than they did previously, and they accordingly return of themselves to their former state on cooling.

This is analogous to the well-known theorem of the dynamical theory of heat, which states that heat can never be transferred from a lower to a higher temperature without the expenditure of work.

Many long-familiar principles at once take their places among the consequences which follow from these considerations. For instance, (1) the heat of combination remains the same whether the combination takes place suddenly at once or by several stages; (2) the heat of combination of a compound body is in general less than the sum of the heats of its constituents.

The following examples may be taken as illustrating the application of the laws of mechanical energy to the explanation of chemical processes.

1. It is well known that the electric spark can occasion the combination of gases in two perfectly distinct ways.

A mixture in equivalent quantities of hydrogen and oxygen, of chlorine and hydrogen, or of carbonic oxide and oxygen, combines suddenly with evolution of heat, and in unlimited quantity, under the influence of a single spark.

Other gaseous mixtures (such, for instance, as nitrogen and oxygen) combine only gradually along the path of the spark itself. No evolution of heat takes place, and the combination of the gases ceases as soon as no more sparks are passed. In the same category we may also place the formation of ozone by means of the electric spark.

In the first case, the energy of the components exceeds that of the compound. A sufficient quantity of energy is therefore at hand; but combination does not occur, because the first condition (a sufficiently strong affinity) is not fulfilled. Under these circumstances, the electric spark increases the power of affinity: combination takes place between a few atoms of hydrogen and oxygen, and the heat thereby developed causes

further combination, since, at least within certain limits, the strength of chemical affinity increases with the temperature.

In other compounds (nitric acid, for example) the components possess less energy than the body formed. In such a case the spark must not only cause a possible increase of affinity, but it must in addition furnish the needful energy; each spark, however, yields only a limited quantity of work, so that the entire mass of such a mixture can never be made to combine by a single spark.

According to Favre and Silbermann*, 1 gramme of hydrogen evolved 7576 units of heat by its conversion into ammonia; ammonia therefore contains less energy than its components. Its behaviour with the electric spark is, however, peculiar. According to Buff†, gaseous ammonia is gradually decomposed by powerful electric sparks; but it is also stated that nitrogen and hydrogen combine under the influence of the spark. But since it is nevertheless impossible that the same spark should produce two completely contrary effects, there must necessarily be some difference between the two cases, which further researches will bring to light. This calls to mind the well-known facts of the decomposition of water-vapour by iron turnings, and the formation of water when hydrogen is passed over oxide of iron‡.

2. Some cases of so-called catalytic action receive a higher explanation on this theory. Platinum causes hydrogen and oxygen to combine to form water, but it does not unite nitrogen and oxygen into nitrous oxide. These facts are connected with the principles that have been developed above. It is evident that the platinum can only modify the affinity; for inasmuch as it does not itself undergo any alteration, it is impossible that it should develope energy. It can only occasion the formation of compound bodies in cases where the required energy is already present in the constituents, and not when, as with protoxide of nitrogen, the constituents contain less energy than the compound. In like manner platinum converts ozone into oxygen, a body which contains less energy, but cannot bring about the opposite change.

It is thus explained how it happens that the electric spark and platinum produce the same effect (formation of water) upon explosive gas, but contrary effects upon ozone and oxygen—the electric spark converting oxygen into ozone, and platinum converting ozone into oxygen.

It is stated that oil of turpentine, when shaken with oxygen,

* *Ann. de Chim. et de Phys.* 3 sér. vol. xxxvii.

† *Fortschritte der Physik*, 1860, p. 501.

‡ [By an obvious oversight the original has *Kupferdrahtspäne* (scraps of copper wire) and *Kupferoxyd* (oxide of copper).—Transl.]

causes the formation of ozone without itself undergoing any modification. Such an action would certainly be catalytic, and would contradict the theory that has been put forward. But it is not difficult to discover a source of energy in this experiment. By shaking, the oil is doubtless heated, since the external work performed must transform itself into heat; accordingly it is only needful that the oil should be heated somewhat less than would otherwise be the case, in order to furnish sufficient energy for the formation of the very small quantity of ozone which results*.

3. In the memoir already cited†, Deville discusses a condition which he calls *Dissociation*, intermediate between a state of firm combination and one of decomposition. This condition is supposed to arise when the molecules are separated from each other to a certain distance. The fact that melted platinum causes the formation of explosive gas when it is thrown into water, is what gave rise to these considerations. In this case the platinum can only act upon the aqueous vapour which is formed. Regnault has shown that at about 1000° C. aqueous vapour is decomposed by melted silver, which absorbs its oxygen. On the other hand, the heat evolved by the combustion of explosive gas suffices for the fusion of platinum, producing a temperature which the author estimates at at least 2500° C. He expresses surprise that the combination of hydrogen and oxygen should develope a much higher temperature than that which occurs in the decomposition. He seeks an explanation in dissociation, in which state he supposes water-vapour to exist at 1000° C. The atoms of chlorine and hydrogen are regarded as already in the state of dissociation at the common temperature.

The author cites, as cases where decomposition occurs suddenly and with evolution of heat, without previous dissociation, the chloride, iodide, and sulphide of nitrogen. Lastly, he compares the three conditions of firm combination, dissociation, and decomposition, with the three states of aggregation.

This last comparison appears to me very defective. In fact there is no great difficulty in seeing that the author's dissociation coincides in general with the state in which only one of the two necessary conditions of chemical action pointed out above is fulfilled. In chlorine and hydrogen there is a sufficient supply of energy, and all that is wanting is an increase in the strength

* [But does the oil of turpentine remain quite unaltered? or is agitation absolutely necessary? There is no apparent reason for supposing that shaking favours the formation of ozone otherwise than by renewing the surface of contact between the oxygen and oil of turpentine. On the other hand, there is reason to believe that *light* plays an important part in the action. (Cf. Schönbein, Chem. Soc. Quart. Journ. vol. iv. p. 135, foot-note.) —Transl.]

† *Fortschritte*, &c. 1860, p. 380. Phil. Mag. S. 4. vol. xx. p. 451.

of affinity, such as day-light, for instance, suffices to produce. The same applies to chloride of nitrogen, &c., and even in the case of the decomposition of water there is nothing that need surprise us.

Water contains less energy than its components. If, therefore, these do not combine, this is due to the non-fulfilment of the first condition, or want of a sufficiently strong affinity. If we represent this affinity as $=0$ when no combination occurs in presence of sufficient energy, in the case before us the affinity is $=0$ at the ordinary temperature, it acquires a certain value at higher temperatures, and it disappears again at a still higher temperature—namely, that at which water-vapour is decomposed by heat alone. It is not decomposed by itself at 1000°; but if it is in contact with melted silver, the metal can, in the first place, furnish the energy needed for the decomposition*; and in the second place it can exert a different action on the two components of the vapour, and thus weaken their mutual affinity. The silver has therefore in this case a twofold action.

Were simple decomposition to occur at 1000°, without anything further, it would certainly be surprising; but in presence of melted silver this is not the case.

The development of a higher temperature by the combustion is quite another question. A product is formed containing less energy than the substances from which it is produced; consequently energy is set free in the form of heat and raises the temperature of the resulting water-vapour. This temperature can be calculated from the known values of the heat of combination of water and of the specific heats of hydrogen, oxygen, and water, and is found to be about 6800° C.† In this calculation, however, it is assumed that no heat is communicated to the surrounding medium. But a temperature as high as this must immediately fall; and when we determine it, it is doubtless already much lower. According to Deville's experiments, however, it may amount, under favourable conditions, to at least 2500° C.

Now the melted platinum can, in the first place, supply the requisite energy; and secondly, the metal very probably acts differently at this high temperature on the two constituents of aqueous vapour. Otherwise this would become a problem of molecular forces.

The phenomena of dissociation cited by the author appear to

[* How? Why should silver at 1000° impart to aqueous vapour more energy than the latter would receive by contact merely with the sides of a porcelain or platinum vessel at the same temperature?—Transl.]

† *Leçons de Chimie et de Physique, professées à la Société Chimique de Paris* (1861), p. 65.

me, therefore, quite explicable by the theory of energy, without the help of any hypothesis concerning the distances of the molecules.

4. Kirchhoff discusses the heat of combination in his memoir on the effective function, and demonstrates that in general it must alter with the temperature*. This follows also as a direct consequence of the following considerations. Let explosive gas be converted into water at different temperatures, *e. g.* at 50° and 100° C., in a closed space, and therefore without development of external work; the heat of combination can be the same in both cases only if the difference between the quantities of energy contained in the water and in the explosive gas at the two temperatures is the same. This again implies that aqueous vapour and explosive gas take up equal quantities of heat when they undergo equal elevations of temperature—in other words, that under constant volume the specific heat of water-vapour is the same as that of explosive gas. This cannot in general be assumed.

Kirchhoff finds for this difference between the specific heats of water-vapour and explosive gas, upon two distinct hypotheses, 0·0417 and 0·212 unit of heat *per* 1° C. We find similarly, in the case of the formation of carbonic acid from carbonic oxide and oxygen, 0·0049 unit *per* degree and *per* gramme.

Since the alteration of the specific heat with the temperature has been determined for only a few substances, this difference cannot yet, in most cases, be exactly stated.

5. The heat of combination is in general regarded as a measure of chemical affinity; but although experiment shows on the whole a greater heat of combination in the case of stronger combinations, many striking exceptions nevertheless present themselves which make it impossible to accept this as a fully proved principle. Thus phosphoric acid has a greater heat of combination than sulphuric acid, although the latter displaces it from its compounds. Potash is a stronger base than lime, but evolves less heat than lime does by union with nitric acid. Oxide of silver neutralizes the properties of acids more com-

* Poggendorff's *Annalen*, vol. ciii. p. 203. [This had been distinctly implied some years previously by Thomsen (of Copenhagen) in his Thermochemical Investigations (Pogg. *Ann.* vol. lxxxviii. p. 349, 1853, No. 3). It is perhaps also allowable to refer to Watts's 'Dictionary of Chemistry,' vol. iii. p. 117, where the probability is pointed out that the thermal effect of a given chemical change is not absolutely constant, but is affected by the circumstances under which the change takes place, and where (before the publication of the present memoir, and in ignorance of what had been said by Kirchhoff) the writer deduces the almost necessary variation of the heat of chemical action with the temperature, from considerations regarding specific heat essentially the same as those insisted on by Kirchhoff (*loc. cit.*) and by the author in the text.—Transl.]

pletely than oxide of copper, and can even displace the latter oxide from its combinations, actions which are dependent on affinity; its heat of combination is, nevertheless, smaller than that of oxide of copper*. It follows from this, that it is not possible to deduce one from another the force of affinity and the heat of combination; we have in fact here to deal with two magnitudes of totally distinct kinds—energy and affinity; and to take one as a measure of the other, seems to me no more allowable than to take the electromotive force of the closed galvanic circuit as equal to the tension of the electrodes when the circuit is open. The heat of combination, denoting the difference between the quantities of energy contained in the reagents and products, is a measure of the stability. Water†, with the heat of combination 29,413, is more stable than hydrochloric acid, whose heat of combination is 23,783. Decomposition can take place only if a quantity of energy equivalent to the heat of combination is restored to the compound; and accordingly decomposition may sometimes be possible in the case of hydrochloric acid, under circumstances in which it is not possible for water. But it does not follow from this that decomposition will actually take place every time this condition is fulfilled; chemical affinity comes also into account, as is shown by the example of explosive gas, where the requisite energy is present in full measure, but no combination occurs at the ordinary temperature.

If the heat of combination must thus be regarded as only a very imperfect measure of the force of affinity, it is impossible

* [None of these examples can be taken as conclusive that the heat of chemical action ought not to be regarded as the measure of the chemical affinity concerned. This will be evident if we examine each in succession.

1. According to Favre and Silbermann, *one equivalent* of potash evolves 16,083 units of heat by combination with 1 equiv. of sulphuric acid, and 17,766 units of heat by combination with 1 equiv. of phosphoric acid (to form the dipotassic phosphate?); according to Andrews, however, the quantities of heat evolved are respectively 15,900 and 14,200. But, admitting Favre and Silbermann's results to be probably the most accurate, it would be difficult to prove that sulphuric acid *does* completely displace phosphoric acid from phosphate of potash.

2. Why is potash called a *stronger* base than lime? If in some cases it displaces lime from its combinations, it is in other cases itself displaced by lime; moreover the remarks made upon the next example apply here also.

3. The fact that oxide of silver can sometimes displace oxide of copper from its salts is not more surprising than that it can displace oxide of hydrogen. The change is in both cases inadequately stated as the mere replacement of one oxide by another; it ought rather to be regarded as a double decomposition in which oxide of silver is decomposed and oxide of copper is formed,—a change which corresponds to the evolution of 15,772† units of heat, and may therefore well compensate a possible absorption of heat in other parts of the process.—Transl.]

† Debray, *Leçons de Chim. et de Phys.* 1861, p. 63.

to take it as any measure at all where combination is accompanied by absorption of heat, and the chemical affinity would therefore be a negative quantity.

In the example of water, already considered, we saw that the affinity, or the tendency to combine, where sufficient energy is present, changes with the temperature, and is accordingly a variable quantity. In like manner it is different in different bodies. If, for example, one acid displaces another from its combinations, the first has a greater affinity for the base than the second. For the most part, however, in such cases as these, as always in precipitation, changes in the distribution of energy take place, whereby the matter is complicated. We can speak of pure effects of affinity only when the quantity of energy is the same before and after the combination. This is always the case, according to Favre and Silbermann, where two salts mutually decompose each other in solution without precipitation. In such experiments, however, it is often very difficult to state how the decomposition occurs.

It is doubtless by no means easy to give a good definition of affinity. That which is commonly given*, namely, that it is the force which brings about the combination of bodies, and which retains the substances thus formed in their new conditions, has the disadvantage of being ambiguous; we may doubt whether both these meanings always coincide. In what has gone before, the affinity of two bodies has always been taken to mean the total effect which they exert upon each other, account having been already taken of the energy. In this respect the definition is more negative than positive.

The application of the theory of energy may perhaps give us a clearer insight into this also. The combinations and decompositions are known experimentally; they depend upon two causes, one of which, the energy, can be determined; and it may thus be possible, from the result and the first cause, to acquire a nearer knowledge of the second. But since this affinity is different between each pair of substances, such investigations as these demand an exact acquaintance with the specific properties of the various chemical substances. The further development of this subject belongs therefore more to chemistry than to physics.

It is worthy of being further remarked that the function of energy in these combinations very often coincides with that of Stahl's *phlogiston*. Just as carbon was considered as containing a large quantity of phlogiston, which escaped when the carbon was burned into carbonic acid, and the amount of which was connected with the heat of combination, so likewise carbon and oxygen in the separate state contain more energy than the

* Favre, *Thèses*, &c., p. 1.

carbonic acid formed from them; and this difference is, as it were, indicated by the heat of combination.

It is obviously impossible to do more in this short communication than to point out the general features of the application of the theory of energy to a few cases of chemical action; but what has been said may suffice to prove the importance of this theorem in the region of chemical phenomena.

Maestricht, March 1864.

Postscript to the foregoing Memoir.*

The principle that the heat of combination cannot be taken as a measure of affinity can perhaps be rendered intelligible as follows. When a freely falling body comes suddenly to rest, we must assume that its *vis viva*, $\frac{1}{2}mv^2$, is converted into heat, provided no other effects are produced. Supposing we could measure this quantity of heat accurately, as well as the final velocity of the falling body, each experiment would furnish a demonstration of the principle of the conservation of energy; and this result would be the same in whatever part of the earth the experiment was made. The variations and other properties of gravity upon the earth would nevertheless remain quite unknown to us.

This case is analogous to the one we are considering. We measure the heat of combination, which may be taken as the measure of the energy or *vis viva* expended, that is to say, of a product one factor of which is the affinity or force of chemical attraction, and the other the change of position which occurs under the influence of this force. This change of position is no doubt much more complicated than in the case above considered, inasmuch as the force cannot be regarded as by any means independent of the mutual positions of the molecules. But as the positions of the molecules are entirely unknown, nothing can be predicated of the forces; and as the properties of gravity in the case supposed above, so here the chemical forces remain quite unknown to us.

The action of these forces manifests itself in the combinations which occur as a consequence of chemical affinity. A stronger acid displaces a weaker one from combination because the affinity of the former for the base is greater than that of the latter. Now in Favre and Silbermann's experiments we certainly find in general the greatest heat of combination developed by the strongest combinations; in some cases, however, we find the opposite. There is nothing at all strange in this; for there is, strictly speaking, no connexion at all between the two magnitudes, and we have reason to be surprised rather at the prevailing agreement than at the occurrence of exceptions.

July 1864.

* Poggendorff's *Annalen*, vol. cxxii. p. 658 (August 1864).

XXXIX. *Elasticity of Vapours.* By W. J. Macquorn Rankine, C.E., LL.D., F.R.SS.L. & E.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

1. MR. ALEXANDER, quoted by Professor Potter in the Philosophical Magazine for February, was surely mistaken when he claimed as *new*, in 1848, the formula

$$p=(a+b\mathrm{T})^6$$

for the pressures of saturated vapours. The general formula of which it is a particular case, $p=(a+b\mathrm{T})^n$, was first proposed by Dr. Thomas Young nearly sixty years ago. The same formula, with the index $n=5$, was used by Arago and Dulong in 1829*, and with the index $n=6$, by Tredgold about 1828†.

2. The history of that and many other formulæ is given by M. Regnault in his *Relation des Expériences &c.*, vol. i. pp. 582 *et seqq*. He gives the preference, for purposes of interpolation, to the formula proposed by Biot in 1844,

$$\log p=a+b\alpha^{\mathrm{T}}+c\beta^{\mathrm{T}},$$

the *five* constants a, b, c, α, β being deduced from five experimental data for each fluid.

3. Young's formula, it is true, contains three constants only, a, b, and n; but, as M. Regnault has shown, it is deficient in exactness. It has, in particular, two faults,—that for a certain temperature, $\mathrm{T}=-\frac{a}{b}$, it makes the pressure of the fluid disappear, and become negative below that temperature, which is exceedingly improbable; and that it makes the pressure of every vapour increase without limit as the temperature rises—a result contradicted by the experiments of M. Regnault, which (as he states in vol. ii. p. 647) point to the conclusion, that *"the elastic force of a vapour does not increase indefinitely with the temperature, but converges towards a limit which it cannot exceed."*

4. The first of those faults, but not the second, exists in Roche's formula,

$$\log p=a-\frac{b\mathrm{T}}{1+m\mathrm{T}}.$$

5. So far as I am aware, no formula has yet appeared, containing *three* constants only, which agrees so closely with experiment as that which I proposed in the Edinburgh Philosophi-

* *Mémoires de l'Intut.*

† Treatise on the eam-Engine.

cal Journal for July 1849, viz.

$$\log p = a - \frac{b}{t} - \frac{c}{t^2};$$

where t denotes the absolute temperature, measured from the absolute zero, 274° C. below melting ice, and a, b, and c are determined from three data for each fluid. (For values of those constants for various fluids, see also the Philosophical Magazine for December 1854, and 'A Manual of Prime Movers,' p. 237). This formula, besides agreeing very closely with experiment at all temperatures, gives the following results:—That every substance can exist in the state of vapour at all temperatures above the absolute zero; and that the pressure of saturation of every vapour tends towards a limit as the temperature increases,—the latter result being in accordance with the conclusion deduced by M. Regnault from his experiments.

6. It may be remarked that if vapours at saturation were perfectly gaseous, it can be proved from the laws of thermo-dynamics that their pressures of saturation would be given by the formula

$$\text{hyp}\log p = a - \frac{b}{(c'-c)t} - \frac{c''-c'}{c'-c}\,.\,\text{hyp}\log t;$$

where c is the specific heat of the gas at constant volume, c' its specific heat at constant pressure, c'' the specific heat of the liquid, b the *total heat of gasefication* of the fluid at the absolute zero, from which t is reckoned, and a a constant to be determined by experiments on the pressure corresponding to a given boiling-point. So far as I know, this proposition has not before been published; but its demonstration will be obvious to any one acquainted with the principles of thermo-dynamics. When the formula is applied to steam, it gives pressures agreeing very closely with actual pressures of steam from 0° to 160° C.; but above the latter temperature the effect of the deviation of the vapour from the perfectly gaseous condition becomes considerable; so that at 220° C. the pressure given by the formula for a perfect gas is about one-fiftieth part less than the actual pressure.

I am, Gentlemen,

Your most obedient Servant,

W. J. Macquorn Rankine.

Glasgow University, Feb. 18, 1865.

P.S.—Since the above was written, I have seen the formula proposed by Mr. Edmonds in the Philosophical Magazine for March 1865. In the notation of the present paper, that formula

is thus expressed:

$$\log p = b\left\{1-\left(\frac{a}{t}\right)^n\right\}.$$

It obviously possesses the same general character with my formula of 1849; viz. it makes the pressure a function of the reciprocal of the absolute temperature, containing three constants, vanishing at the absolute zero, and converging towards a limit when the temperature increases indefinitely; and it is satisfactory to me to see that Mr. Edmonds, by an independent investigation, has arrived at a result which thus agrees in the main with mine.—W. J. M. R.

March 1865.

XL. *Sir Charles Lyell and the Glacial Theory of Lake-Basins.* *By* A. C. Ramsay, *F.R.S., V.P.G.S.**

IN Sir Charles Lyell's new edition of the 'Elements of Geology,' he has devoted several pages to the discussion of the theory of the "connexion of the predominance of lakes with glacial action," and he does me the honour, by a number of counter arguments, to combat the views I advanced in 1862 †. In the opening passage he adopts that part of my reasoning in which I first insisted, as a significant fact, on the connexion of multitudes of lakes with regions high and low in all latitudes that have been thoroughly subjected to ice-action, and their comparative rarity in countries where the signs of glacial action have not been observed‡. It is with satisfaction that I now see

* Communicated by the Author.

† "On the Glacial Origin of certain Lakes," &c., Journal of the Geological Society, vol. xviii. p. 185 (Proc. March 5, 1862).

‡ On this subject Sir Charles observes, "It has been truly remarked that lakes are very common in those countries where erratics, striated boulders, and rock-surfaces, with other signs of glaciation, abound, and that they are comparatively rare in tropical and subtropical regions. When travelling over some of the lower lands in Sweden, far from mountains, as well as over the coast-region of Maine in the United States, and other districts in North America, I was much struck with the innumerable ponds and small lakes, of which counterparts are described as equally characteristic of Finland, Canada, and the Hudson's Bay territories." These are the very regions to which I directed attention in my Memoir of 1862; and my attention having been directed by a geologist of distinction to the passage quoted, I was led to infer that by accident I had done injustice to the published views of Sir Charles Lyell. It was therefore with a sense of relief that, on referring to his two journeys in North America and other writings, I failed to find any allusion to the subject. I mention it now lest others should draw the same inference that I did. As far as I am aware, I first drew special attention to the fact in connexion with the Glacial Theory.

this fact stated as a piece of common knowledge in a manual so popular as Sir Charles Lyell's is sure to be. Some physical geologists may doubtless marvel that Sir Charles, writing of lakes that "run in great rents and faults," is still of opinion that the existence of such rents and faults in connexion with valleys "is no more than may be said of most of the longitudinal and transverse valleys in every mountain-chain;" but I will not argue that point, and I may forget the assumption when I find it coupled with the admission of the truth of the principle I endeavoured to establish, that mountain-lakes do not lie in gaping fractures, and that fractures, wherever we know them by eye, are almost always close*. Sir Charles, I am glad to see, also approves of my argument to show that the Alpine and other lake-basins are not the result of special subsidences; and the admission of all these points by him will help no doubt by-and-by to procure the adhesion of readers who do not think or have no opportunity of observing for themselves. Those who go so far take so many steps in the right direction—steps, I think, which may in the long run lead them to accept my theory altogether.

But though there is this partial agreement in some details, including the direct power of ice "in scooping out shallow basins where the rocks are of unequal hardness" (Antiquity of Man), Sir Charles does his utmost to disprove the possibility of glaciers on a great scale having been the means of scooping out by slow erosion large lake-basins, such as those of the Alps, Scotland, Sweden, or North America; and I now propose, as briefly as I can, to examine some of the arguments to which he seems to attach the greatest weight.

The erosion, then, of large rock-basins is untenable because even if ice, in descending a steep slope, "scoop out one of those cavities called tarns," yet we must suppose that it loses all power of extending the cavity, being unable to cut *a gorge* through the lower margin of the tarn; and "this diminished force of erosion, wherever the ice has to ascend a slope, *or to move horizontally*, seems adverse to the hypothesis of the formation of lakes of considerable length and depth by glaciers." In my last paper, published in the Philosophical Magazine for October 1864, some months before the appearance of the 'Elements,' I discussed for the second time what I believe to have been the peculiar scooping effects of huge glaciers that issued from the slopes of great, yet comparatively narrow valleys into the wider plains that they overspread, or into flats near the

* Attributed by Sir Charles to Mr. Jukes, who in an admirable article in the 'Reader,' March 12th, 1864, used in my favour, and with new illustrations, the arguments which I employed in my original memoir.

mouths of the valleys themselves, and still within their bounds. And though Sir Charles has not met the arguments urged either in my first or second paper, except by implication, I am constrained by the circumstances of the case to repeat them in a manner that I hope cannot be misunderstood. This may be done in a very few words. Every physicist knows that when such a body as glacier-ice descends a slope, the direct vertical pressure of the ice will be proportional to its thickness and weight and the angle of the slope over which it flows. If the angle be 5°, the weight and erosive power of a given thickness of ice will be so much, if 10° so much less, if 20° less still, till at length, if we may imagine the fall to be over a vertical wall of rock, the pressure against the wall (except accidentally) will be *nil*. But when the same vast body of ice has reached the plain, then motion and erosion would cease, were it not for pressure from behind (excepting what little motion forward and sidewards might be due to its own weight). This pressure, however, must have been constant as long as supplies of snow fell on the mountains, and therefore the inert mass in the plain was constantly urged onwards; and because of its vertical pressure its direct erosive power would necessarily be proportional to its thickness, and greater than when it lay on a slope; for it would grate across the rocks, as it were, unwillingly and by compulsion, instead of finding its way onwards more or less by virtue of gravity. Indeed the idea is forced on the mind, that the sluggish ice would have a tendency to heap itself up just outside the mouth of the valley and there attain an unusual thickness, thus exercising, after its descent, an extra erosive power. Further, as I have said elsewhere, when the glacier spread well out upon the plain, far beyond the mouth of the valley, it would of necessity thin more and more by melting; and this seems to me a very obvious reason why, its weight being lessened, the waste of underlying matter by erosion would decrease towards what are now the mouths of those lake-basins which Sir Charles, following the supposition of the late Dr. Falconer, allows were filled with ice during the glacial period. These propositions seem to me so obvious, that I should scarcely have thought it necessary to restate them; but if they be mere fallacies, it is singular that no one has yet thought it worth while to refute them. Sir Charles himself seems to allow that the ice may have had "to ascend a slope"*.

The remark that in a "part of a valley from which a glacier has retreated in historical times, no basin-shaped hollows are conspicuous," is met, if we think of it, by the foregoing

* To discuss the details of this subject would involve a repetition of what I have already printed, and this I must necessarily avoid. Geological Journal, *l. c.*; Phil. Mag. October 1864, &c.

observations; for the extreme end of the petty glaciers of to-day have only a small erosive power, and not one that I know in the Alps has ever in historical times been protruded in mass *on and well over a plain.* When a glacier lies on a slope, it needs little reflection to show that its tendency will by no means be to produce "cup-and-saucer-shaped cavities"*.

The argument that the constant occurrence of transverse rents in the same part of a glacier proves that the ice "cannot saw through and get rid of the obstacles which impede the freedom of its onward march," reads strangely after the admission that "everywhere we behold proofs that the glacier, by the aid of sand and pebbles, can grind down, polish, and plane the bottom." Neither the mountains that bound nor the valleys under the glaciers can possibly preserve the same relative details of feature till all shall be worn away; and the position of the obstacles as constants can no more be considered indestructible than the Falls of Niagara, which Sir Charles Lyell has long ago shown may retreat till Lake Erie itself shall be drained.

Another point seems to require explanation. At the beginning of the subject (p. 168) it is stated that lakes are *exceedingly common in all regions* that have been glaciated, and rare in tropical and subtropical regions; and at page 170 it is observed that "such basins, large and small, are met with *in all latitudes.*" Now I have specially guarded myself against being supposed to assert that all lake-basins have been formed by ice; but if "such basins, large and small, are met with in all latitudes," which I doubt (except among mountains which maintain or have maintained glaciers), their relative proportions in different latitudes deprives the argument of much or of any value; and for other reasons the same may be said of the remark "that there are lacustrine deposits of *all geological epochs,* attesting the existence of lakes at times when no one is disposed to attribute them to the agency of ice." There may have been lakes of all geological epochs; but I should like to see proofs adduced; and very few of them are mentioned in the 'Elements.' Where are the lakes of the Silurian strata, which themselves embrace more epochs than one, if unconformities constitute epochs? And though Sir Charles compares the Old Red Sandstone fish to

* See my paper "On the Erosion of Valleys and Lakes," Phil. Mag. October 1864. I have often thought that the absence or scarcity of lakes on the southern flanks of the Himalayah is due to the well-known steepness of the valleys, and their occurrence in numbers on the north is owing to the opposite circumstance. Tarns I know there are on fragmentary flats on the mountains on the south side. But I can only judge from maps and descriptions, and therefore dare not positively assert it. After the publication of my first memoir, Dr. Hooker wrote me a letter confirmatory of these views, which were till then new to him.

living genera in African and American rivers, is he prepared to follow Mr. Godwin-Austen's opinion, that much of the Old Red Sandstone, as distinct from Devonian rocks, is a lacustrine deposit? With respect to the Coal-measure strata, constant reference is made in the 'Elements' to their formation in deltas or lagoons; but no mention is made of great deep inland lakes. Indeed the word *lake* is only once used in the description of this formation, and it is immediately qualified by the word *lagoon*. Has any one yet described Permian lakes? though I believe they will be found. And even in his account of rock-salt, Sir Charles does not assert that the salt of the Trias was formed in far inland continental lakes supersaturated with salt, though he refers to those of Asia; and he again insists rather on lagoons, as in the Runn of Cutch or the Bahr Assal, near the Abyssinian frontier, or the possibility of salt now forming in the Red Sea. Are there any Liassic, Oolitic, or Cretaceous lakes described? On the contrary, all their freshwater formations are either said to be deltoid, or the manner of their formation is left in the dark. It is true that lakes have been described of very late Eocene (?) and of Miocene age; and there the record of them begins and ends till we come to post-pliocene and recent times. It is therefore by no means yet a piece of common knowledge "that there are lacustrine deposits of all geological epochs."

But if "lacustrine deposits" are "of all geological epochs," has no one spoken of "the agency of ice" in past times? or has no one written of anything that might suggest that idea to an unbiased mind? Let us look to this. Mr. John Carrick Moore has described conglomerates in the Lower Silurian rocks of Wigtonshire, which might well be called boulder-beds; for a prodigious number of the enclosed masses of gneiss and granite (which Mr. Moore has pointed out to me on the ground) range from a foot up even to six feet in diameter, and all of them have been derived from ancient strata (perhaps Laurentian) of a region now unknown.

The conglomerate of the Old Red Sandstone of several parts of Scotland and the North of England is so like the "Upper boulder-drift" of many parts of Britain, though consolidated, that other geologists besides myself have spent hours in searching it for scratched stones; but, for chemical reasons connected with pressure, which Mr. Sorby will appreciate, none have yet been found, if they ever existed there*.

Mr. Godwin-Austen has been so bold as to attribute the transport of blocks in the French carboniferous rocks to floating ice; and I invite any one to examine the ice-scratched erratics

* The Rev. J. G. Cumming long ago suggested the glacial origin of the Old Red conglomerates of the Isle of Man.

of the Permian strata in the Jermyn Street Museum, and then to judge if the subject as described by me does not, to say the least, deserve the measure of attention which it has received in the Manuals of Professor Phillips*, Professor Jukes, and Mr. Page.

I feel convinced that the same conclusions which I drew for the Rothliegende of part of England will yet be extended to much of that of Northern Germany; for though marls and gravels are interstratified with it, these, as in our post-pliocene drift, are exceptional, and the main characteristic of this vast formation (2600 feet thick) in the Thüringerwald is the flattened and subangular nature of its blocks, some of which are of large size. Similar erratic deposits are now forming in Baffin's Bay and the Western Atlantic.

Mr. Godwin-Austen long ago suggested the ice-borne character of great blocks in the New Red Sandstone of Devonshire; and the Oolitic strata of the east of Scotland contain such numbers of huge angular blocks, that their possibly though scarcely probably glacial origin constantly suggested itself to my mind when I noted the facts during a journey with Sir Roderick Murchison in 1859. The local character of the blocks, chiefly but not entirely Oolitic, is adverse to the view; but the *smashed* condition of many of the shells in the interstratified oolitic clays is analogous to the state of the shells in the upper drifts all over Britain.

It may not be generally known that Escher von der Linth is aware of boulders in the cretaceous strata of the Alps, and Godwin-Austen has suggested a similar origin for boulders sometimes found in the British chalk; and surely, though unnoticed by him in the 'Elements,' Sir Charles is conversant with the clear-sighted observations of Gastaldi, who attributes the formation of certain conglomerates (with scratched stones), and the transport of the huge boulders that lie in them, to the agency of floating icebergs that, descending into a miocene sea, broke from Alpine glaciers, and carried their freights to the neighbourhood of what is now Turin, from the far-off region where the Lago Maggiore at present lies.

Geologists, then, some of them of the highest eminence, have actually written of "the agency of ice" in several geological epochs; and, whether in these epochs or in others mentioned above, it is clear that erratic- and boulder-phenomena not easily to be accounted for exist in many formations, these phenomena being not unlike those that are brought about by floating ice in the present day. The subject of the ancient agency of glaciers and floating ice is indeed far too prominent to be disposed

* Professor Phillips does not agree with me, but still in a note he takes care to notice my opinion.

of without examination, and rejected, for all but post-pliocene time, in half a dozen lines, as if indeed, even in a Manual of stratigraphical geology, the older strata exhibited no perplexing phenomena that might induce anything to be said on the subject worth attention. In the case of the Miocene ice-work of the Alps, which, having seen it with Gastaldi, I have long considered to have been *proved* by him, some persons may consider it suggestive *that lacustrine phenomena do occur* on the flanks of the Alps in the same formation; and if any of the boulder-conglomerates of the Old Red Sandstone be ice-formed, and Mr. Godwin-Austen's suggestion be true, the conjunction occurs again. "It would, indeed, be the most perplexing of all enigmas," says Sir Charles, "if we did not find that lake-basins were now, *and had been at all times*, a normal feature in the physiognomy of the earth's surface, since we know that unequal movements of upheaval and subsidence are now in progress, and were going on at all former geological epochs." Here again we find the assumption of "lake-basins at all times," just as if it were a fact familiar to geologists that such lake-basins had always existed, whereas, eliminating lagoons, the statement seems to be only derived from two or three circumstances relating to strata of tertiary times. Traces of estuarine beds are more frequent; but this is another matter altogether. To me the absence of lake-deposits is not at all perplexing,—first, because the preservation of all superficial terrestrial phenomena (as opposed to marine) has been, for obvious reasons, rare in the world's history, except in strata of very late date; and secondly, because I believe that the conditions for the formation of innumerable lakes like those of North America, Scandinavia, the Highlands, the Alps, and other glacier mountain-regions, were probably comparatively rare in the earlier history of the earth. That accidental lakes, due to volcanos, and a few of them perhaps to unequal movements of upheaval and subsidence, may have existed at all times is perhaps certain; and it would do no harm to my theory were I to concede that all the known and accepted lakes of Miocene and Eocene (?) times, and older ones if they existed, were formed by the processes to which Sir Charles adheres.

These preliminary points regarding past times being stated lead, in the 'Elements,' to the special discussion of Sir Charles Lyell's proposition as to what was in his opinion the real cause of the formation of the larger lakes that flank the Alps; for, except in a vague manner, he does not grapple with the origin of the unnumbered lake-basins that are strewn over the face of such a country as North America. "We need but little reflection," he remarks, "to discover that when changes of level are in progress, some of the principal valleys can hardly fail to be

converted in some parts of their course into lakes of considerable magnitude," because otherwise we should have to assume "that the greatest elevatory movement always conforms to the central axis of every chain," or to that "of every watershed." "But sometimes upheaval will be in excess in the lower part of the valley, and at other times (which would equally produce lake-basins) there would be an excess of subsidence in the higher regions, the alluvial plains below sinking at a less rapid rate, or being, perhaps, stationary." And here I must be allowed to remark that these considerations did not escape me when I wrote my memoir "On the Glacial Origin of certain Lakes"; but I rejected them (I now see, unwisely) as random surmises, not comparable in value to the various hypotheses I discussed, and as I believe disproved, viz., that the great Alpine lakes "lie in simple synclinal troughs," or in "areas of mere watery erosion," or in mere "lines of dislocation," or "in areas of special subsidence." I shall now show why I rejected and still reject both of the above suppositions proposed in the 'Elements.'

Referring to 'The Antiquity of Man,' Sir Charles very properly assumes that the large valleys of the Alps were of pre-glacial origin,—a good and a sound assumption, founded on definite proofs as old as the days of Charpentier, if not older, and one I had occasion in this Magazine to show may have originated in subaërial actions that have been going on ever since the close of Miocene times*. No one, therefore, ever dreamed that "the rivers had been idle for a million of years or more, leaving to glaciers the task of doing, in comparatively modern times, the whole work of excavation." But the question he proposes to solve is, how, controverting my proposition of glacier-erosion, parts of these valleys may have been converted into lakes. Let us take the Lago Maggiore as an example.

If in this case upheaval was "in excess in the lower part of the valley," what would the result be?

From the deepest part of the lake to its efflux is a distance of about twelve miles, and the average angle from the deepest part to the efflux is $2^\circ 21'$, and, giving every advantage to any one who prefers upheaval "in excess in the lower part of the valley," I will assume that the axis of movement lay on a line coincident with the deepest part of the lake, or, in other words, that the hinge, so to speak, of the movement lay there. Before the upward movement began, the whole slope must have been downward towards the valley of the Po, otherwise the drainage would have been dammed up; and it needs little reflection to see in that case that the point which is now the efflux of the lake must have been 2625 feet, or say 2650 feet lower than at present, so

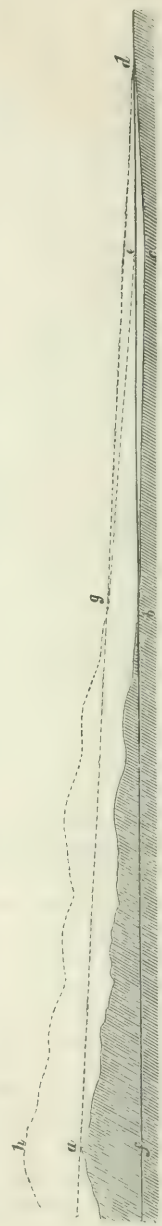
* Phil. Mag. November 1862.

as to bring it at the least to a level a little lower than what is now the deepest part of the lake. Without this the onward southern flow of the water could not have been established. But in that case the plain of the Po outside the present efflux of the lake must also have been at the least 2650 feet lower than at present, that is to say, before the tilting began; in which case the plain must have been about 2000 feet below the level of the present sea, and liable to be covered with deposits of that post-pliocene epoch. But no trace of these deposits exists, and they have never even been imagined; for the post-pliocene deposits of the valley of the Po are older than the ancient glaciers. The only escape from this is to suppose that when these movements took place the whole region lay so high (from 2000 to 3000 feet higher than at present) that elevations and depressions had no immediate relation to existing sea-levels.

But though Sir Charles alludes to upheaval "in excess in the lower part of the valleys," he rejects it in the case of the Alpine valleys, and prefers another hypothesis to account for the actual existence of the lakes as they now stand; and this I shall now examine.

"The Alps," he observes, "are from 80 to 100 miles across. Let us suppose a central depression in this chain at the rate of 5 feet in a century, while the intensity of the movement gradually diminishes as it approaches the outskirts of the chain, till at length it dies out in the surrounding lower region." Thus in time the valley-slopes that originally all declined outwards and downwards from the central elevations of the mountains, would in the lower regions, by depression of the central ridges, by degrees acquire a reverse slope, that is to say, towards their ends they would slope inwards to the mountains, and by this process the drainage would become dammed up by rock, and lake-basins would be formed. Now, to test this idea, we must take the distance between the efflux of lake and lake on the opposite sides of the chain. From the outflow of the Lago Maggiore to that of the lake at Lucerne, the distance is roughly about ninety-three miles in a straight line; and if we measure another line as far as the north end of the Lake of Zurich, the distance is about 112 miles. It will make no material difference in my argument which line I take; but let us take the latter, for it is clear that the rule of subsidence ought to apply to the lakes in general on both sides of the Alps. A point halfway between the outflow of the Lake of Zurich and that of Maggiore lies near the upper end of the valley of the Rhone, that is to say, just about the main centre of the Alpine chain. The distance, then, from the south end of the Lago Maggiore to the central point of subsidence in the Alps was about fifty-six miles. Now the

Lago Maggiore from end to end is about thirty-three miles long, if we disregard the curve of the lake, and from its efflux to the Borromean Isles, where it is deepest, is twelve or thirteen miles, and the average upward slope of the bottom of the lake to its outflow from that point is about 2° 21′. If, then, the chief line of depression lay in the centre of the Alps, and if that depression was the cause of the formation of the lake, then it is evident that, before the bottom of the lake assumed its present form, the whole region, from its outflow to the centre of the Alps, must have been so tilted, that the present upward slope from the Borromean Isles to the efflux must have sloped the other way (viz. south) at some angle, however small. And here I must have recourse to a diagram; for experience has shown me that many admirable geologists are yet exceedingly apt to exaggerate or else to neglect their angles. It is, as near as may be, on a true scale. Let *a* be the crest of the Alps, say 14,000 feet high, *b* the northern end of the lake, *c* its bottom, 2625 feet deep, *d* its efflux, and *b d c* an angle of 2° 21′, viz. the slope of the lake from *c* to *d*. What we have got to do is to alter the general levels of the country by a maximum upheaval at *a*, so that the line *c d*, instead of sloping upwards from *c* to *d*, shall slope downwards a little in the opposite direction, viz. from *e* to *d*. The depth at *c e* is 2625 feet; and to give the argument every point against me, let the axis of the movement lie at *d*. There the actual movement will be *nil*; and for every mile you proceed towards *a* the amount of upheaval will increase. To restore the country to its original form, as supposed by Sir Charles, let the point *c* be raised 2625 feet because of a general tilt of the solid country comprised between the lines *a d c f*, so as to raise the triangle *b c d* into the position of the triangle *g e d*. Then it so happens that the line *d f* at *f* will be raised as near as may be to the point *a*; or, in other words, the crest of the Alps would be raised to *h*, and the whole range in this neighbourhood, at the period



before depression began, must have been 28,000 or 30,000 feet high during the greatest extension of the glaciers. If we make an allowance for denudation, of course the Alps were still higher. Depress the central ridge *h* till it attains the height of *a*, the axis of movement or hinge being at *d*, and the triangle *g e d*, previously filled with ice, would assume the position of the triangle *b c d*; and when the glacier melted, the hollow became filled with water*. This is asking a good deal; and if it were necessary to account for the greater snowfall of the old glacier-epoch in the Alps by increased height of the mountains, which it is not, though we might be inclined to grant Charpentier his 3000 feet, the difficulty increases when we are asked to grant an elevation five times as much; nor indeed is the question likely to be raised by any one who measures his angles and calculates his numbers. If we remove the point where the angular movement ceased out into the alluvial plain of the Po, the difficulty simply increases for every yard we carry it in that direction; and a little reflection will show that at no great distance the angular movement necessary to drain the lake would raise the Central Alps to a height of 60,000 instead of 30,000 feet†.

Neither have we any special reason to suppose that any of those oscillatory movements have frequently taken place in the Alps, such as are common accompaniments of earthquakes in volcanic areas; and the trifling instances Sir Charles gives of these in Cook's Strait, and of another gradual movement in Finmark of 135 feet, though they have some relation to the subject, yet cease to have any probable significance when we consider the magnitude of the movement needed in our case, and also that it is not only necessary to apply it for the formation of the rock-bound lake-basins of the Alps, but also to numerous other cases on the flanks of many mountain-chains, and not there alone, but to the widely glaciated undulations of North America. Has Dr. Julius Haast also been mistaken when, adopting generally my views, he accounts for the excavation of the rock-bound lake-basins of New Zealand by glaciers? or was there a central depres-

* For the sake of simplicity, I make no allowance for curvature, which would be very small even if the problem were reduced to extreme accuracy. To attempt this would be merely pedantic.

† If, however, we are seriously asked to grant the probability of such movements having taken place in the Alps at a geological period so late, it is difficult to see why Sir Charles Lyell should object to Professor Heer's hypothesis, that Europe and America were joined across the Atlantic when the Miocene flora grew in Europe. The depth of the Atlantic is not so great but it would be easy to carry a line across it in soundings not greater than the oscillations of level I have indicated the Alps are required to have undergone during and since glacial times. If any one can grant this for times so recent, it is easier to grant it for times so old as the Miocene epoch.

sion of the mountains there also? Does this theory of depression apply to the Scandinavian chain and the Swedish lakes, upon which Dr. Torrel told me my theory threw so much light? Is Sir Charles prepared, if necessary, to apply it to the Vosges and the Black Forest? Will it meet the case of the lakes of the Pyrenees, which, I am informed, quite conform to my views? and were the greater rock-basins of Cumberland formed by a depression of the centre of that cluster of mountains, so that, instead of the lakes ranging on either side of a line of strike, they all radiate outwards from a centre? How will it suit Loch Lomond, Loch Katrine, and Loch Awe in Scotland, or, better still, Loch Ericht, Loch Rannoch, and Loch Lydach, which stand towards each other something like the legs of an old Isle of Man penny? and what of all the other myriad lakes of the Highlands, which trend east and west, south-east and north-west, north and south, and to every point of the compass, in accordance with the run of the valleys that gave a direction to the flow of the old glaciers? Were the marine lochs—once glacier-filled land-valleys—that open south into the Clyde, west into the Atlantic, and north into the North Sea, and which are generally deepest (like Loch Lomond) towards their heads—were they also tilted up at their ends by depression of the inland mountains? for no one who studies them is likely to assert that they are shallower nearer their mouths by mere gathering of sediment. And what about the lakes on the north watershed of the Himalayah? Is there no risk that we may be obliged to add 15,000 or 20,000 feet to the stature of that gigantic range to meet the exigencies of the case? When we come to the mountains and the rolling undulations of North America, where vast tracts are covered with unnumbered lakes, many of which are as large as those of Switzerland, what a variety of tiltings in all directions must have been produced to dam up basins, the outflows of which run towards every point of the compass! It must be remembered, too, that Sir Charles's supposition may be applied equally to these tracts, whether the lakes are entirely enclosed by rocks, or were dammed up by moraines and drift after the disappearance of the glaciers.

If, however, it be objected that the tilting that produced the great lakes on the south side of the Alps was not the result of a special sinking of the centre of the chain, let us take another supposition, viz. that the main line of depression lay on an east and west line, on the parallel of the north end of the Lago Maggiore and the Lago di Como; then, if the hinge or axis of movement lay east and west on a line at or near the south end of these lakes, the amount of depression for the north end of Maggiore would be about 7000 to 8000 feet, and that of Como

would approach the lowest of these numbers; while if we shorten the line still more, and place it at the Borromean Islands, we then get merely a special sinking of the bottom of the lake, —that for Maggiore, making no allowance for sediment, being 2625 feet. Considering, however, that the depression of the chain, according to Sir Charles Lyell's hypothesis, *ought to have been the means of forming the rock-basins on both sides of the Alps*, it is difficult to get rid of the idea that a great central depression was the cause, if there be any ground for the idea at all. In that case Charpentier's 3000 feet come very far below the mark. Indeed, if we must allow 14,000 feet of depression to form the lake-basins (still full of ice), one cannot see why there are not a great many more lakes than we find; for they ought also to occur in other valleys that run north and south of the central chain and open on the plains, but which are merely river-courses.

Again, how do the existing lakes conform to the regulation? Certainly those of Geneva and Neuchâtel do not in their trend agree with a supposed depression of the Central Alps; for their lengths and outflows are, roughly, at right angles to those of the other great lakes of Como, Lugano, Maggiore, Orta, Varese, Garda, Thun, Lucerne, Zug, Sempach, Zurich, and Constance; and to dam up the lakes of Geneva and Neuchâtel, we should require a central depression running north-west between them at right angles to the chain of the Alps, and quite across the Miocene rocks. For this we need a special proof, which has never even been attempted; and I do not see but that to produce the whole of the lakes by depression, the supposed great movement must merely resolve itself into a number of minor ones. A better supposition than this seems to me to be a special dislocation, or a special depression for each lake, which I have elsewhere attempted to disprove.

And now, to sum up the matter, let us see what I am expected to allow if I am forced to accept as proved the adverse points that have been raised against my theory. The Alps "*may have been* at the time of intense cold 3000 feet higher than they are now. They *may also* have been lower again." "The repetition of such unequal movements *may*, in a time geologically brief, turn parts of a valley into a lake." "*If there be no ice* during the movement, &c.," and "*should the movement* be very slow," "the river *may* afterwards fill up the cavity," and "*it may* afterwards cut through the new stony barrier." "*If the change* takes place in a glacial period, the thickness of the ice will augment from century to century, not in consequence of erosion, but simply because the contour of the valley is becoming gradually more basin-shaped. The mere

occupancy, *therefore*, of cavities by ice, by preventing fluviatile and lacustrine deposition, *is one cause* of the abundance of lakes which will come into existence whenever the climate changes and the ice melts." After so many *ifs*, and *shoulds*, and *may be's*, I submit that *may be one cause*, instead of *is one cause*, would have been a more appropriate mode of expression. These suppositions are backed by the additional statement, that if "we observe a capricious distribution of lake-basins, we have no reason to feel surprise, so long as we conceive the origin of such basins to be due to subterranean movements in the earth's crust; for these *may be* partial in their extent, or *may vary* in their direction in a manner which has no relation to the course of the valleys." I prefer to so many surmises the simplicity of my hypothesis, that as glacier-ice does erode the rocky floor over which it passes, and as it can, under certain circumstances, move up slopes, the nature of that erosion will be, and was dependent,

1. On the angles of the slopes over which it passed, when these slopes were seriously appreciable.

2. On the fact that the glaciers sometimes passed from these slopes into low grounds, into which the great old glacier-valleys opened.

3. That at the mouths of these great old valleys, and sometimes near their mouths, where two or more great glaciers met, the downward pressure of all the accumulated ice of all the tributary valleys would be greatest.

4. For, because of its inertness in such flat ground, the grinding-power of the ice urged on from behind would be greatest, in accordance with all known physical laws; and

5. That, as it progressed and melted, the ice must have been thinner, and must have exercised less erosive power than where it was thick, whence the gradual slope of the bottom of these lakes towards their outflows.

Sir Charles does not deny that glacier-ice may move up a slope. His idea of tilting supposes it, for the lake-hollows were not filled up with sediments, because they were filled with ice; but, apart from this, on one side there is immense complication of phenomena, which, to meet his case, must be applied to all the mountain-chains and clusters I have ever seen, and, as far as I know, to all of them I have ever read about, and, besides, to all the length and breadth of the northern half of North America, while on my side there is, at all events, simplicity.

As for the surmise that icebergs are likely to hollow out lake-basins of any importance in solid rocks, I have already discussed the subject; and it is so immaterial to the main argument, and seems to me so utterly improbable, that I will not at present renew the discussion.

XLI. *On an Extension of the Dynamical Principle of Least Action.* *By* JAMES H. COTTERILL, *St. John's College, Cambridge* *.

WHEN a material body, or system of bodies, is exposed to the action of force, the points of application of that force move to a greater or less degree, and the motion of these points of application, by reason of the physical connexion of the points of the system, causes a general relative displacement of every one of them, thereby calling into play forces which increase with the extent of that displacement, and at length become sufficiently great to balance the applied forces; the displacement then ceases, and a state of equilibrium is attained.

Thus in every case of the balance of forces by the resistance of matter, a certain amount of energy is expended by the applied forces, and a certain amount of work done in partially overcoming the resisting forces; and it is a well-known case of the general law of the conservation of energy, that the energy so expended is equal to the work so done.

But further, if any portion of the system be considered apart from the rest, the forces generated at the points of junction must form a system of forces in equilibrium, and the energy expended by them considered as applied to the detached portion must be equal to the work done in that detached portion; and these are general conditions to which the forces generated at every point of the system are subject. Now Mr. Moseley has shown that if any number of pressures are in equilibrium, some of which are resistances, then each of these resistances is a minimum, subject to the conditions imposed by the equilibrium of the whole—a principle which he has called the principle of Least Resistance; let us assume this principle, and let us further suppose, for the present, that it is generalized so as to include the case of the resisting forces generated as above described; then each of those resisting forces is a minimum, subject to the general conditions stated above: and further, the relative displacements which are the cause of those forces must also be the least possible, and the *work done* the least possible. Thus in the assumption mentioned, to which I shall return in the sequel, it appears that the work done is a minimum, subject to the law of conservation of energy and the statical conditions of equilibrium; and this principle, analogous to the dynamical principle of Least Action, it is the object of this article to consider and apply.

If the work done be expressed in terms of the resisting force at all the points of the system, or some of them, then, the law of

* Communicated by the Author.

conservation of energy being implicitly satisfied, we have simply to make the work done a minimum, subject to the statical conditions of equilibrium.

The principle in question is (theoretically) sufficient to determine the law of distribution of the stress within an elastic body exposed to given forces; but if we attempt to apply it by expressing the work done in an element of the body in terms of the elastic forces by the known formula given by Clapeyron, and then make the integral of the result a minimum by Lagrange's method, subject to the well-known differential equations expressing the equilibrium of that element, we shall simply fall upon the general equations given by Lamé; the method, therefore, being of no practical value, I shall not dwell upon it.

There are, however, a number of questions of practical importance, in which it is required to find the stresses on the several parts of a structure composed of beams, chains, pillars, and the like. In these cases the work done in the structure can, on hypotheses more or less perfectly realized in practice, be expressed in terms of the stresses at certain parts of the structure; the ordinary method of maxima and minima then furnishes the values of the stresses. And here the application of the principle seems to me to possess some advantage. Mr. Moseley has given some formulæ for the work done in a beam acted on by given forces, but they are not so convenient for the present purpose as one which I shall presently give; I first, however, premise the following demonstration of a slight modification of the formula (510) in his work on Engineering and Architecture (first edition):—

Let a couple M turn through an angle i, then the energy expended is Mi if M be constant throughout the angle; but if M be producing a gradual deformation of a perfectly elastic body, so that M varies as i, then will the work done be $\frac{1}{2}$ Mi. Now consider the condition of a transverse slice of a beam made by planes originally parallel at a distance dx, but now, in consequence of a bending moment, M acting on it, inclined at an angle di, which, as is shown in all works on applied mechanics, is equal to $\frac{M}{EI}dx$, where E is the modulus of elasticity, and I the moment of inertia of the sectional area; the energy expended in distorting it is $\frac{1}{2}$ Mdi, or $\frac{M^2}{2EI}dx$, and therefore, by the law of conservation, the work done in the slice is $\frac{M^2}{2EI}dx$, and the work done in the whole beam $\int \frac{M^2}{2EI}dx$, a formula which is

equivalent to the one referred to. Next, let a beam of uniform section be under the action of a transverse load of uniform intensity w per unit of length, and also of bending moments M_1, M_2 acting on its extremities *against* the load; then, if the bending moment at a point distant x from one end be expressed in terms of M_1, M_2, w, x, and c, the *half* span of the beam, the result on being squared and integrated between 0 and $2c$ gives for U, the work done,

$$U = \frac{c}{3EI}\{M_1^2 + M_1 M_2 + M_2^2 - wc^2 (M_1 + M_2) + \frac{2}{5} w^2 c^4\}.$$

By aid of this formula the work done in a uniform beam, acted on by any transverse forces in one plane and a uniformly distributed transverse load, can be estimated by a process of addition in terms of the bending moments at the points of application of the forces. If the forces are not transverse, then the following theorem is necessary:—The work done in a beam by a thrust and a bending moment is the sum of the works done by each of them acting separately.

For the bending moment does not at all affect the length of the line drawn through the centres of gravity of its several transverse sections, since that line lies in the neutral surface; and, on the other hand, the work done by the thrust, considered as uniformly distributed over the sectional area, is equal to the work done by its resultant which passes through the centres of gravity, and consequently that work is unaffected by the bending moment. It follows, therefore, that if H be the thrust and A the sectional area, $U = \int \left\{ \frac{M^2}{2EI} + \frac{H^2}{2EA} \right\} dx$.

The hypotheses employed in these formulæ are those which are usually employed in treating of the strength of beams, namely, that the strain is within the limits of perfect elasticity, and that the effect of the tangential stress is inconsiderable. I shall now give some examples of the method of applying the principle to the questions I referred to; but as it is very uniform, two will suffice.

1. A beam is fixed horizontally at both ends, and loaded with a vertical load distributed uniformly; find the bending moments at the fixed ends. Here, if w be the intensity of the load, M_1 M_2 the required bending moments, and c the half span, the work done in the beam is given above, and we have simply to make it a minimum by variation of the unknown quantities M_1, M_2, whence

$$2M_1 + M_2 - wc^2 = 0, \quad 2M_2 + M_1 - wc^2 = 0,$$
$$M_1 = M_2 = \tfrac{1}{3} wc^2.$$

This result agrees *exactly* with that given by the ordinary

method, although both methods are only approximate, the reason of which will be seen from what follows. Let $i_1\, i_2$ be the slopes at the extremities of a beam acted upon by $M_1\, M_2$ at its extremities, and by the uniform load w, then U being, as shown above, an homogeneous quadratic function of M_1, M_2, we have

$$2U = \frac{dU}{dM_1} M_1 + \frac{dU}{dM_2} M_2 + \frac{dU}{dw} w\,;$$

but by the law of conservation

$$2U = M_1\, i_1 + M_2\, i_2 + w\, u,$$

where u is the area of deflection of the beam; and comparing these expressions, we see that

$$\frac{dU}{dM_1} = i_1, \quad \frac{dU}{dM_2} = i_2, \quad \frac{dU}{dw} = u\,;$$

but the ordinary method is founded on the consideration that the beam is horizontal at its extremities, in other words, that $i_1 = 0$, $i_2 = 0$; so that the two methods lead to the same result by the same equations. And this will be the case in all questions concerning continuous beams; but the present method enables us to obtain the requisite equations by differentiation of a single function.

2. A suspension-bridge platform is stiffened by the application of a girder of uniform section; find the bending moment at its centre when loaded in the middle.

Here, to obtain a complete solution, the work done in the whole structure, chains, piers, suspending-rods, and girders, must be estimated in terms of the bending moments at the centre and the points of attachment of the rods, and the tensions of those rods. To simplify, suppose the rods indefinitely many in number (as is usual in suspension-bridge questions), and the tension per unit of length of the girder w; also suppose the form of the chain, before the load is put on, parabolic, so that w would be uniform if the load produced no distortion, and will actually be sensibly uniform, because the distortion is inconsiderable. These suppositions being made, the work done in half the girder would be

$$\frac{c}{3EI}\{M_1^2 + M_1 M_2 + M_2^2 - w c^2 (M_1 + M_2) + \tfrac{2}{5} w^2 c^4\},$$

where c is the *quarter* span, M_1 the bending moment at the extremity of the girder, M_2 at its centre. I shall consider the case in which the girder is attached, but not fixed at its ends, in which case $M_1 = 0$, and the expression becomes

$$\frac{c}{3EI}\{M_2^2 - w c^2 M_2 + \tfrac{2}{5} w^2 c^4\}.$$

In the first place, suppose the chains and rods sensibly inextensible, and the piers sensibly immoveable, then the only

work done in the structure is that done in the girder, and we must make it a minimum, subject to the statical condition connecting $M_2 w$ and the central load W, which, taking moments about one end, is found to be

$$\frac{W}{2} \cdot 2c - 2w c^2 - M_2 = 0,$$

or

$$M_2 + 2w c^2 = Wc.$$

Differentiating both equations, we have

$$(2M_2 - w c^2)\, d M_2 + (\tfrac{4}{5} w c^4 - M_2 c^2)\, dw = 0,$$
$$d M_2 + 2c^2\, dw = 0.$$

Therefore, for a minimum,

$$2M_2 - wc^2 = \tfrac{2}{5} wc^2 - \tfrac{1}{2} M_2,$$
$$\tfrac{5}{2} M_2 = \tfrac{7}{5} wc^2;\ M_2 = 1\tfrac{4}{5} wc^2;$$

but

$$M_2 + 2wc^2 = Wc,$$

so that

$$M_2 = \tfrac{7}{32} Wc;\ wc = \tfrac{25}{64} W;$$

or, if l be the span ($l = 4c$),

$$M_2 = 1\tfrac{7}{128} wl;\ wl = \tfrac{25}{16} W.$$

These results agree exactly with those obtained by a writer in the 'Civil Engineer and Architect's Journal' for 1860, and differ slightly from those given by Prof. Rankine in his work on 'Applied Mechanics.' To find v, the central depression of the girder, we have, by the law of conservation,

$$2U = Wv;$$

but if we eliminate M_2 from the expression for U by aid of the equation connecting M_2, W, w, U will then be a homogeneous function of Ww, so that

$$2U = \frac{dU}{dW} w + \frac{dU}{dw} w;$$

comparing which expressions for U, we have

$$v = \frac{dU}{dW} = \frac{dU}{dM_2} \cdot \frac{dM_2}{dW} = c \frac{dU}{dM_2};$$

$$\therefore v = \frac{c^2}{3EI} \{2M_2 - wc^2\}$$
$$= \frac{c^2}{3EI} \left\{ \frac{28}{25} wc^2 - wc^2 \right\} = \frac{wc^4}{25EI}$$
$$= \frac{Wl^3}{4096EI}.$$

Secondly, suppose the elasticity of the chains and rods to be taken into account, then the work done in stretching them must be estimated in terms of w, and added to the expression for U, thereby introducing terms of the form $k w^2$, where k is a quantity depending on the rise, span, and elasticity of the chains; U must then be made a minimum as before.

Having briefly indicated the mode of applying the principle of least action to various problems, I return to its demonstration.

From the description given of the process by which equilibrium is attained, it is apparent that if the principle of Least Resistance be given, the principle of Least Action follows, and *vice versâ*; and since the principle of Least Resistance is well known, I have assumed it. But inasmuch as that principle has perhaps never been satisfactorily proved, at least in its general form, it will be well to give a direct demonstration of the principle of Least Action in the case where the body is perfectly elastic.

Let X, Y, Z be the components of one of the forces acting on a free perfectly elastic body, u, v, w the displacements of its point of application parallel to three rectangular axes, U the work done in the body, then

$$2\mathrm{U} = \Sigma\,(\mathrm{X}u + \mathrm{Y}v + \mathrm{Z}w)\,;$$

but U may be expressed as a homogeneous quadratic function of the forces; therefore

$$2\mathrm{U} = \Sigma\left\{\mathrm{X}\frac{d\mathrm{U}}{d\mathrm{X}} + \mathrm{Y}\frac{d\mathrm{U}}{d\mathrm{Y}} + \mathrm{Z}\frac{d\mathrm{U}}{d\mathrm{Z}}\right\};$$

comparing which expressions for U, we see that

$$\frac{d\mathrm{U}}{d\mathrm{X}} = u, \quad \frac{d\mathrm{U}}{d\mathrm{Y}} = v, \quad \frac{d\mathrm{U}}{d\mathrm{Z}} = w.$$

Now conceive the body, instead of being free, to be immoveably attached at certain points to some fixed object, then we shall have for these points

$$\frac{d\mathrm{U}}{d\mathrm{X}} = 0, \quad \frac{d\mathrm{U}}{d\mathrm{Y}} = 0, \quad \frac{d\mathrm{U}}{d\mathrm{Z}} = 0\,;$$

that is, the variation in U, due to a change in the resisting force at the fixed boundaries of the system, is zero. And the same is true for a change in the resisting force anywhere within the mass; for conceive the body to be divided into two parts by a surface of any form passing through the point, and let U_1, U_2 be the works done in the two portions, then $\frac{d\mathrm{U}_1}{d\mathrm{X}}$ and $\frac{d\mathrm{U}_2}{d\mathrm{X}}$ are evidently equal and of opposite sign, that is, $\frac{d\mathrm{U}}{d\mathrm{X}} = 0$, as before.

Since, then, the change in U, consequent on any possible change in the resisting forces, is zero, U must be a minimum (the other two possible hypotheses being easily seen to be inadmissible), and the principle is proved for a perfectly elastic body or system of bodies.

In default of a more general demonstration, it may perhaps, by many persons, be considered self-evident that, *cæteris paribus*, energy must be expended in that way in which there is least opposition to such expenditure, and that, on the other hand, work will be done, *cæteris paribus*, in that way in which most energy can be expended. And is it not a general law of energy, that the energy expended is a maximum, and the work done a minimum, subject to the laws of transformation of energy, whether general or peculiar to the particular case under consideration?

11 Barnsbury Villas, Liverpool Road,
March 3, 1865.

XLII. *Chemical Notices from Foreign Journals.* By E. Atkinson, *Ph.D., F.C.S.*

[Continued from vol. xxviii. p. 235.]

BERTHELOT* has found that when acetylene and iodine are heated together at 100° for fifteen to twenty hours, they combine and form a crystallized iodide which has the formula $C^2 H^2 I^2$.

A saturated solution of hydriodic acid absorbs acetylene slowly, and forms a liquid dihydriodate which is about twice as heavy as water, and is volatile at 182° without special decomposition. Its formation is thus expressed:—

$$C^2 H^2 + 2\,HI = C^2 H^4 I^2.$$

It is thus isomeric with iodide of ethylene, than which it is much more stable.

Both the iodide and the dihydriodate of ethylene yield acetylene when treated with alcoholic potash.

Berard† has found that acetylene, when allowed to bubble through an ethereal solution of iodine, is slowly absorbed, and a combination of iodine and acetylene is formed. This compound, however, is more easily obtained when silver-acetylene suspended in water is shaken with ethereal solution of iodine until the iodine is no longer decolorized. After distilling off the ether, the residue solidifies, forming a mass of yellowish crystals which

* *Comptes Rendus*, vol. lviii. p. 977.
† Liebig's *Annalen*, July 1864.

have the composition $C^4 H^2 I^4$, and are thus formed by the union of two molecules of acetylene. It has a very unpleasant odour, is volatile at ordinary temperatures, but decomposes when melted. By treatment with various agents it furnishes acetylene. Treated with reducing agents it furnishes a very volatile oil, which is probably iodated acetylene, $C^2 H^4 I$, corresponding to the brominated acetylene of Sawitzsch and of Reboul.

The author is engaged on the further investigation of this and of other substances.

Semenoff* recommends for the preparation of ethylene gas a mixture of 1 part by weight of absolute alcohol and 5 parts by weight of sulphuric acid. With such proportions the addition of sand is unnecessary; for with careful heating in the sand-bath there is no frothing up, and the disengagement of gas, which commences at 100°, is quite regular.

For the preparation of monobrominated ethylene, $C^2 H^3 Br$, the same author recommends the following method. In a retort connected with a Liebig's condenser is placed a mixture of bromide of ethylene, $C^2 H^4 Br^2$, with an equivalent quantity of aqueous potash; alcohol is then dropped in from a burette until two layers are no longer perceptible. At the temperature of 40° to 50°, the monobrominated ethylene is then distilled off mixed with alcohol. It is obtained in almost the theoretical quantity.

Friedel† has described a new method of preparing allylene‡. In the year 1857 he described two compounds obtained by the action of chloride of phosphorus on acetone, *methylchloracetole*, $C^3 H^6 Cl^2$, and *chlorpropylene*, $C^3 H^5 Cl$. The latter he has found is readily converted into allylene. It was enclosed in a sealed tube with sodium-alcohol, and heated for about 16 hours to a temperature of 120°. The tube was then provided with a vulcanized tube and carefully opened. A quantity of gas escaped, which was collected over brine, in which it is less soluble than in water. Its properties were found to agree in all respects with those of allylene.

Oppenheim§ has investigated the action of bromine and of iodine on allylene-gas. The method used by him is that of its discoverer Sawitzsch||, that is, the action of sodium-alcohol on

* *Zeitschrift für Chemie*, No. 5, 1864.
† *Bull. Soc. Chim. Paris*, vol. ii. p. 96.
‡ Phil. Mag. vol. xxi. p. 359.
§ *Répertoire de Chimie*, July 1864.
|| Phil. Mag. S. 4. vol. xxi. p. 359.

monobrominated propylene. Several attempts to discover other and easier methods for its preparation have not been successful.

When bromine is brought into contact with allylene gas in the shade, a limpid mixture of two bromides is obtained, which can be separated and obtained pure by distillation *in vacuo*. If the action takes place in the light, substitution-products are obtained.

The first of the above bromides (the bibromide of allylene, $C^3 H^4 Br^2$) is a colourless liquid of a sweetish odour, but whose vapours are very irritating to the eyes. Its density is 2·05 at 0°. It boils at 132°, and is thus readily distinguished from its isomers, dihydrobromic glycide, which boils at 152°, and bibrominated propylene, which boils at 120°.

The *tetrabromide of allylene*, $C^3 H^4 Br^4$, forms the greater part of the product of the action of bromine on allylene. It is a colourless liquid, with a pronounced camphorous odour. Its density is 2·94 at 0°, and it boils between 110° and 130° under a pressure of a centimetre.

Oppenheim finds that iodine combines with difficulty with allylene when the two are exposed in a stoppered vessel to the direct action of the sun. The action is promoted by the use of a solution of iodine, either in bisulphide of carbon or in iodide of potassium. Biniodide of allylene, $C^3 H^4 I^2$, is a colourless liquid which decomposes on distillation. Acted upon by bromine, it gives rise to the above bromide.

Liebermann* has found that when a solution of iodine acts on silver-allylene, a volatile oil with an unpleasant odour is formed, which has the composition $C^3 H^3 I$, and is therefore iod-allylene. On lengthened contact with iodine this substance takes up two atoms of iodine, becoming converted into the body $C^3 H^3 I^3$, which is colourless and almost without odour.

De Luynes found that when erithyrite, $C^8 H^{10} O^8$, was treated with hydriodic acid, it was changed into a liquid which has the composition of iodide of butyle, $C^8 H^9 I$; it is not, however, identical with this body, but is rather $C^8 H^8$, HI, a compound of butylene with hydriodic acid.

When the above compound is treated with acetate of silver, it yields a liquid boiling between 111° and 113° which has the composition of acetate of butyle, and another boiling at about 5°. The latter is butylene gas, first obtained by Faraday from the decomposition of fatty substances. De Luynes† gives its properties as follows. Gaseous at ordinary temperatures; it has a peculiar odour. It is but very partially soluble in water, but very much so in

* Liebig's *Annalen*, July 1864.
† *Comptes Rendus*, vol. lvi. p. 1175.

ether; when its ethereal solution is diluted with water, the gas is given off with violent effervescence. It burns with a red blue-edged fuliginous flame. It is tolerably soluble in crystallizable acetic acid. It is dissolved by concentrated sulphuric acid; and on diluting this acid with much water, a less dense, pleasantly odorous liquid is separated. It is absorbed by hydriodic acid, with which it forms hydriodate of butylene.

Butylene can be condensed to a liquid, and its boiling-point is then found to be $+3°$, and not $-8°$ as given in the books. It combines with bromine to form bromide of butylene, $C^8 H^8 Br^2$.

In a subsequent paper the same chemist has furnished a further description of the properties of hydriodate of butylene, $C^8 H^8, HI$, and hydrate of butylene, $C^8 H^8, H^2 O^2$. He shows that they bear the same isomeric relations to the iodide of butyle, $C^8 H^9 I$, and butylic alcohol, $C^8 H^{10} O^2$, that Wurtz found existing between hydriodate of amylene, $C^{10} H^{10}, HI$, and hydrate of amylene, $C^{10} H^{10}, H^2 O^2$, on the one hand, and iodide of amyle, $C^{10} H^{11} I$, and amylic alcohol, $C^{10} H^{12} O^2$, on the other.

Schöyen* describes as follows the synthesis of butyric acid. He first prepared ethyle gas by a modification of Frankland's method. This was then mixed with the same volume of chlorine and exposed to bright daylight. An action was immediately set up with the formation of hydrochloric acid gas, and an oily liquid which, after appropriate rectification, was found to consist of chloride of butylene, $C^4 H^8 Cl^2$, along with substitution products richer in chlorine and chloride of butyle, the formation of which is thus expressed:—

$$\underset{\text{Ethyle.}}{C^4 H^{10}} + Cl^2 = HCl + \underset{\substack{\text{Chloride of}\\ \text{butyle.}}}{C^4 H^9 Cl}.$$

The whole of the distillate under 90°, consisting essentially of this body, was enclosed in sealed tubes with acetate of potash and acetic acid, and heated to 100°. Chloride of potassium separated out, and a colourless oily liquid which, on rectification, was found to be acetate of butyle, $\left.\begin{matrix} C^4 H^9 \\ C^2 H^3 O \end{matrix}\right\} O$, containing, however, chlorine compounds. By treatment with baryta, this acetate was decomposed for the purpose of forming butylic alcohol, and then the product was oxidized with chromic acid. In this way an aqueous distillate was obtained which had the characteristic odour of butyric acid, the further identity of which was proved by converting it into the lime and silver and mercury salts.

* Liebig's *Annalen*, May 1864.

By treating cyanide of ethyle with potash, Frankland and Kolbe obtained propionic acid; and this reaction has been since then very frequently used for passing from one alcohol to the acid corresponding to the next higher homologous alcohol. Claus* has succeeded in applying this reaction to the preparation of crotonic acid, $C^4 H^6 O^2$, starting from cyanide of allyle, $C^3 H^5 CN$. The preparation of this latter substance was readily effected by the action of cyanide of potassium on iodide of allyle. The cyanide of allyle was then boiled in a retort connected with an inverted condenser, until no more ammonia was disengaged, and the odour of cyanide of allyle was no longer perceptible. The contents of the retort were then neutralized with carbonic acid, boiled, and filtered to remove carbonate of potash. The filtrate was evaporated to dryness, and repeatedly crystallized to get rid of the last portions of carbonate. The potash-salt thus purified was decomposed by means of dilute sulphuric acid and distilled, by which means an aqueous acid distillate was obtained that was used for the preparation of the other salts. The decomposition of cyanide of allyle by potash is as follows:—

$$\underset{\text{Cyanide of allyle.}}{C^3 H^5 CN} + KHO + H^2 O = \underset{\text{Crotonate of potash.}}{C^4 H^5 KO^2} + NH^3.$$

The aqueous distillate thus obtained is a colourless pungent acid liquid, with an odour resembling that of butyric acid, from which the acid separates at 0° in small white crystals.

The salts show in general great resemblance to the corresponding compounds of acrylic acid.

Michaelson has investigated propylic and butylic aldehydes†. He heated a mixture of butyrate and formiate of calcium in the expectation of forming butylic aldehyde, in accordance with the following reaction,

$$\underset{\text{Formiate of calcium.}}{C H CaO^2} + \underset{\text{Butyrate of calcium.}}{C^4 H^7 CaO^2} = \underset{\text{Butylic aldehyde.}}{C^4 H^8 O} + \underset{\text{Carbonate of calcium.}}{C Ca^2 O^3},$$

but found that along with the expected substance propylic aldehyde, $C^3 H^6 O$, was also formed.

About 80 grammes of the mixed salts were used in one operation. The product of the reaction was treated with oxide of lead, and then rectified over chloride of calcium. It began to boil at 62°, and at 90° two-thirds remained in the retort. The distillate, on rectification, gave a product which boiled between 54° and 63°. The results of the analysis and vapour-density determination of this substance agreed with those required for

* Liebig's *Annalen*, vol. cxxxi. p. 58.

† *Bull. Soc. Chim. Paris*, vol. ii. p. 123.

propylic aldehyde, $\left.\begin{matrix}C^3H^5O\\H\end{matrix}\right\}$; and that the substance was not acetone, which has the same formula, was proved by the fact that it reduced silver with formation of propionic acid. The residue in the retort, as mentioned above, was found on rectification to boil between 73° and 77°. The analysis and the vapour-density determination of this substance, together with its chemical deportment, left no doubt that it was butylic aldehyde, C^4H^8O.

The same chemist has investigated the products of the oxidation of butylic alcohol. He found it impossible, by fractional distillation of fousel oil, to obtain a pure butylic alcohol. But by converting the fractions containing most butylic alcohol into iodide and fractionizing these, he obtained pure iodide of butyle. This was first converted into acetate of butyle, and this, by decomposition with potash, into butylic alcohol.

When this butylic alcohol was oxidized with bichromate of potash and sulphuric acid, it yielded propylic and butylic aldehydes, and carbonic, propionic, and butyric acids. Hence chromic acid oxidizes not merely the hydrogen, but also part of the carbon, which is converted into carbonic acid.

When acetic or benzoic acid or their homologues are distilled with excess of alkali, they decompose into carbonate and a hydrocarbon which contains an atom of carbon less than the acid: thus benzoic acid, $C^7H^6O^2$, gives carbonic acid, CO^2, which combines with the base, and benzole, C^6H^6. Harnitz-Harnitzsky* has succeeded in performing the inverse operation of passing from benzole to benzoic acid. Chlorocarbonic acid was passed into a retort, which was heated and exposed to the light at the same time that benzole vapour also entered. This retort was connected with a condensing arrangement. The reaction took place only with the vapour of benzole; it was accompanied by the disengagement of hydrochloric acid gas.

The product of the reaction was separated by distillation from the unattacked benzole. It was found to boil constantly at 198°. This fact and its other properties left no doubt that it was chloride of benzoyle; thus

$$\underset{\text{Benzole.}}{C^6H^6} + \underset{\text{Chlorocarbonic acid.}}{COCl^2} = \underset{\text{Chloride of benzoyle.}}{C^7H^5OCl} + HCl.$$

When this substance was dropped in water it sank to the bottom, and was gradually changed into a crystalline mass which

* *Comptes Rendus*, vol. lviii. p. 748.

was readily identified as benzoic acid, the formation of which takes place as follows:—

$$\underset{\text{Chloride of benzoyle.}}{C^7 H^5 O Cl} + H^2 O = \underset{\text{Benzoic acid.}}{C^7 H^6 O^2} + HCl.$$

Tollens and Fittig* have described the synthesis of a number of hydrocarbons of the benzole series. It is an extension of Wurtz's method of obtaining in the fatty-acid series, the mixed radicals by the union of two different alcohol radicals.

When to a mixture of monobromobenzole† and iodide of methyle and ether sodium was added, a brisk reaction was established which had to be moderated. After distilling off the ether at 60°, almost the entire quantity passed over between 108° and 116°, and, after one or two rectifications over sodium, was found to boil at 111°. This substance has the composition of *methyl-phenyle*, $C^7 H^8 = \left.\begin{matrix} C^6 H^5 \\ C\ H^3 \end{matrix}\right\}$. It is a colourless liquid, resembling benzole in its odour, and with the specific gravity 0·881. It resembles in all respects the toluole from coal-tar, not merely in its physical but in its chemical properties; for the authors have easily obtained from methyl-phenyle the derivatives characteristic of toluole, nitrotoluole, and toluidine; like toluole, too, it yields benzoic acid by oxidation; so that there is no doubt as to the identity of the two substances.

By treating a mixture of iodide of ethyle and bromobenzole with sodium, Tollens and Fittig obtained the mixed radical *ethyl-phenyle*, $\left.\begin{matrix} C^6 H^5 \\ C^2 H^5 \end{matrix}\right\}$, which has the same composition as xylole. It forms a nitro-compound, $C^8 H^9 (NO^2)$, which, when reduced, yields a crystallizable base.

In like manner a mixture of bromobenzole and bromide of amyle yields a mixed radical, *amyl-phenyle*, $\left.\begin{matrix} C^6 H^5 \\ C^5 H^{11} \end{matrix}\right\}$. It boils at 195°, and forms, when treated with fuming nitric acid, a nitro-compound, $C^{11} H^{15} (NO)^2$. By oxidation with a concentrated solution of chromic acid, it yields a mixture of acids difficult to separate, the greater part of which is benzoic acid.

Berthelot and De Luca, by treating iodide of allyle with sodium, obtained a new hydrocarbon, $C^3 H^5$, which they called *allyle*. Wurtz has made an extended series of investigations, of which this substance is the starting-point‡. According to Wurtz, its

* Liebig's *Annalen*, September 1864.
† Phil. Mag. vol. xxv. p. 218.
‡ *Répertoire de Chimie*, 1864.

formula should be doubled, $(C^3H^5)^2 = C^6H^{10}$; and he accordingly names it *diallyle*. It is best prepared by treating iodide of allyle with an alloy of 1 part of sodium and 2 of tin.

When diallyle is treated with strong hydriodic acid in closed vessels at a high temperature, combination ensues, but the body formed, *dihydriodate of diallyle*, $C^6H^{10}, 2HI$, cannot be distilled without decomposition. It is a transparent heavy liquid, usually coloured by the presence of a little free iodine.

When this substance is treated with potash, an immediate reaction takes place with deposition of iodide of potassium. The liquid products of the action are *monohydrate of diallyle*, the formation of which takes place thus—

$$C^6H^{10}, 2HI + KHO = H^2O + KI + C^6H^{10}, HI,$$

and diallyle.

Sodium decomposes the dihydriodate with the formation of hydrocarbons, of which hexylene is the principal. *Dihydrochlorate of diallyle*, $C^6H^{10}, 2HCl$, is prepared by heating diallyle with concentrated hydrochloric acid.

When the dihydriodate is treated with acetate of silver mixed with ether, a reaction sets in accompanied by the formation of iodide of silver, which is complete at the expiration of twenty-four hours. The ethereal solution filtered from the iodide of silver was distilled; after the ether had passed over mixed with a little diallyle, the residue was found to consist of free acetic acid and of two acetates of diallyle, one of which boils at 154°, and the other at over 200°. The latter is a *diacetate*,

$$C^6H^{10}\left\{\begin{matrix}H^2\\(C^2H^3O^2)^2\end{matrix}\right.,$$

corresponding to the dihydriodate, and formed in accordance with the equation

$$C^6H^{10}2HI + 2C^2H^3AgO^2 = 2AgI + C^6H^{10}, H^2(C^2H^3O^2)^2.$$

Dihydriodate of diallyle. Diacetate of diallyle.

The former is a monoacetate, $C^6H^{10}\left\{\begin{matrix}H^2\\HO,(C^2H^3O^2)\end{matrix}\right.$, corresponding to the monohydriodate.

When the mixture of these two acetates is treated with caustic potash, a dihydrate is obtained. It boils between 210° and 220°, and is remarkably stable; its composition,

$$C^6H^{14}O^2 = C^6H^{10}\left\{\begin{matrix}H^2\\(HO)^2\end{matrix}\right.,$$

is the same as that of hexylic glycol. When this dihydrate is treated with strong hydrochloric acid or with hydriodic acid, the dihydrochlorate or dihydriodate are respectively obtained.

Besides these biatomic compounds of diallyle, there is a monoatomic series, the starting-point of which is the monohydriodate of diallyle, $C^6 H^{10} HI$, obtained from the dihydriodate by the loss of one molecule of HI.

For the reduction of nitro-compounds, Zinin first introduced the use of sulphuretted hydrogen; and Béchamp subsequently showed that a mixture of iron filings and acetic acid is an extremely energetic reducing agent. Sulphuretted hydrogen acts in such a manner that a single atom of hyponitric acid, NO^2, is replaced by NH^2; while acetic acid and iron filings act so that all the NO^2 present is replaced by NH^2. Beilstein* has recently made some experiments on the reducing action of a mixture of tin and hydrochloric acid. He finds that its action is very energetic; in all cases the whole of the hyponitrous acid, NO^2, is converted into the group NH^2. Thus nitrosalicylic acid is converted into amidosalicylic acid, in accordance with the equation

$$\underset{\text{Nitrosalicylic acid.}}{C^7 H^5 (NO^2) O^3} + 6Sn + 6HCl = \underset{\text{Amidosalicylic acid.}}{C^7 H^5 (NH^2) O^3} + 2H^2O + 6SnCl.$$

In a similar manner dinitrotoluole, $C^7 H^6 (NO^2)$, is converted into *toluylene-diamine*, $C^7 H^6 (NH^2)^2$; and picric acid, $C^6 H^3 (NO^2)^3 O$, into *picramine*†, $C^6 H^3 (NH^2)^3$.

XLIII. *On the Constitution of Chromium-Compounds.* By J. Alfred Wanklyn‡.

THE researches of Deville and Troost on the vapour-densities of the metallic chlorides have led to various attempts to revise the atomic weights of most of the metals. One of the most obvious ways of dealing with the case of iron and of the different metals belonging to the iron family (including of course chromium), was to represent the metal as a six-atomic element, *i. e.*

$$Fe^{vi} = 112; \quad Cr^{vi} = 105$$
$$Fe Cl^6 \qquad Cr Cl^6.$$

This view, which has been advocated by several chemists, involves, among other inconveniences, a very complex formula for the protochlorides, or else the assumption that the protochloride is an unsaturated compound.

In a paper published in the spring of 1862§, Erlenmeyer

* Liebig's *Annalen*, May 1864.
† Phil. Mag. vol. xxv. p. 540.
‡ Communicated by the Author.
§ *Zeitschrift für Chem. und Pharm.*, p. 129.

proposed to look upon ferric chloride as a compound of the second order of complexity, and showed that on this supposition the atomic weight of iron might be 28 or 56.

Among other possible views of the constitution of iron-compounds (and he extended them to the other metals of the iron-family), Erlenmeyer proposed this :

$$Fe^{iv} = 56.$$

1 . . $Fe\ Cl^4$ Hypothetical.

2 . . $Fe^2\ Cl^4$ Ferrous chloride.

3 . . $Fe^2\ Cl^6$ Ferric chloride.

Of course, on the same principle, chromium-compounds would be represented thus :—

$$Cr^{iv} = 52{\cdot}5.$$

1 . . $Cr\ Cl^4$ Hypothetical.

2 . . $Cr^2\ Cl^4$ Chromous chloride.

3 . . $Cr^2\ Cl^6$ Chromic chloride.

In atomicity these metals would be like carbon, compounds 2 and 3 being chlorine representatives of the iron and chromium ethylene, $Fe^2\ H^4$, and ethyl-hydride, $Cr^2\ H^6$.

When either these hypothetical chlorides shall have been discovered, or representatives of them shall have been found, this theory will have acquired claims on the attention of chemists.

There is a chromium representative of the hypothetical chloride. It is chlorochromic acid.

The analysis and vapour-density of chlorochromic acid agree with the formula $Cr\ O^2\ Cl^2$. That chlorochromic acid and the proto- and sesqui-chlorides of chromium are of different orders of complexity is proved by the difference in their boiling-points. If anyone maintains the formula of sesquichloride of chromium to be $Cr\ Cl^3$, he is at once involved in this absurdity, that there are two chromium-compounds differing only in this, that in the one there is a certain atom of chlorine, and in the other this certain atom of chlorine is replaced by O^2, but that whilst the former boils at a red heat, the latter (with the oxygen in it) boils a little above the boiling-point of water.

It is therefore quite certain that, whilst chlorochromic acid contains only Cr in the standard volume, sesquichloride of chromium must contain at least Cr^2.

From the close analogy subsisting between sesquichloride of chromium and sesquichloride of iron (the vapour-density of which requires $Fe^2\ Cl^6$), it also follows that the molecule of chromic chloride (sesquichloride of chromium) must be $Cr^2\ Cl^6$.

The relation of chlorochromic acid to chromic chloride having

been thus satisfactorily disposed of, the next question relates to the function of the O^2 in the chlorochromic acid. Has this O^2 a two or a four saturating power? The well-known oxidizing energy of those compounds, such as Brodie's organic peroxides, which are admitted to contain $(O^2)''$, leads us to see in the extraordinary energy of chlorochromic acid an argument that the oxygen is $(O^2)''$.

Thus chlorochromic acid becomes

$$Cr \left| \begin{matrix} Cl \\ Cl \\ (O^2)'' \end{matrix} \right.,$$

which is the analogue of

$$Cr \left| \begin{matrix} Cl \\ Cl \\ Cl \\ Cl \end{matrix} \right.$$

Carrying out the theory systematically, we should write the chromates on the same type: thus

$$Cr \left| \begin{matrix} (O^2)'' \\ KO' \\ KO' \end{matrix} \right.$$

is neutral chromate of potash.

The establishment of the rational formulæ of chromium-compounds is of course an important step towards the establishment of the rational formulæ of analogous compounds. It must have weight in fixing the constitution of the aluminium-compounds, the formulæ of which have been recently (as it appears to me with little probability) deduced from vapour-density determinations of aluminium-ethyle and aluminium-methyle taken at temperatures at which these bodies have undergone decomposition.

In place of the hypothesis of Messrs. Buckton and Odling, that aluminium-methyle is not a true gas at 130° C. but becomes a true gas at above 200° C., an hypothesis which appears to me to be very unphilosophical, I would suggest that aluminium-methyle, if it should not experience an ordinary decomposition at about 200° C., may perhaps react upon the mercury of the bath, or, more interesting still, resolve itself into

$$2\,Al^2\,Me^6 = 3\,Al\,Me^4 + Al.$$

Either of these suppositions would be compatible with all the data which are as yet published.

London Institution,
March 17, 1865.

XLIV. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF GREAT BRITAIN.

Feb. 3, 1865. "ON Aluminium Ethide and Methide." By William Odling, M.B., F.R.S.

The symbols by which the atomic proportions of a few of the principal metallic elements are usually represented, together with the relative weights of these several proportions, are shown in the following Table:—

Lithium.....	Li	7
Magnesium...	Mg	24
Zinc	Zn	65
Arsenic	As	75
Silver	Ag	108
Tin	Sn	118
Mercury	Hg	200
Lead	Pb	207
Bismuth	Bi	210

It is observable that the atomic proportions of the metals range from 7 parts of lithium, through 108 parts of silver up to 210 parts of bismuth. Now it is found that all these different proportions have substantially the same specific heat, so that 7 parts of lithium, 108 parts of silver, and 210 parts of bismuth, for instance, absorb or evolve the same amount of heat in undergoing equal increments or decrements of temperature. Hence, taking silver as a convenient standard of comparison, the atomic proportion of any other metal may be defined to be that quantity of the metal which has the same specific heat as 108 parts of silver.

Many of the metals unite with the halogen radicals chlorine and bromine, as also with the organic radicals ethyle and methyle, to form volatile compounds, which may be conveniently compared with the chloride and ethide of hydrogen. Now it is found that the several proportions of metal or hydrogen contained in equal volumes of these gaseous chlorides or ethides, are their respective atomic proportions; so that equal volumes of chloride or ethide of hydrogen, zinc, arsenic, tin, mercury, lead, and bismuth, for instance, contain 1, 65, 75, 118, 200, 207, and 210 parts of hydrogen or metal respectively. Hence the molecule of chloride of hydrogen, HCl, being conventionally regarded as constituting two volumes, the atomic proportion of a metal may be defined to be that quantity of the metal which is contained in two volumes of its gaseous chloride, or bromide, or ethide, or methide, &c.

These two definitions having reference respectively to the specific heats of the metals, and the molecular volumes of their gaseous compounds, lead in all cases to the same conclusion. Thus 200 parts of mercury is the quantity of mercury which has the same specific heat as 108 parts of silver, and is also the quantity of mercury contained in two volumes of mercuric chloride, mercuric ethide, &c.

The atomic proportions of the different metals unite with 1, 2, 3, 4, &c. atoms of chlorine and ethyle, to form the two-volume molecules of their respective chlorides and ethides, as shown below :—

2 vols.	2 vols.	2 vols.
H Cl	H Et	
$HgCl^2$	$HgEt^2$	$ZnEt^2$
$Bi\ Cl^3$	$Bi\ Et^3$	$AsEt^3$
$Sn\ Cl^4$	$Sn\ Et^4$	$PbEt^4$

Or, two volumes of the gaseous chlorides of hydrogen, mercury, bismuth, and tin, for instance, are found to contain respectively 35·5 parts, twice 35·5 parts, three times 35·5 parts, and four times 35·5 parts of chlorine.

Aluminium, which is one of the three most abundant constituents of the earth's crust, and the most abundant of all its metallic constituents, enters into the composition of a large number of native minerals of great value in the fine and useful arts, and also forms extremely well-defined artificial compounds, possessing a high degree of chemical interest. Nevertheless chemists are not at all agreed as to the atomic weight which should be accorded to the metal, or as to the molecular formulæ of its principal compounds.

The quantity of aluminium which has the same specific heat as 108 parts of silver, is found to be 27·5 parts; and analysis shows that this quantity of aluminium combines with three times 35·5 parts of chlorine to form chloride of aluminium. Accordingly the atomic proportion of aluminium should be fixed at 27·5 parts; its chloride be formulated as a trichloride thus, $AlCl^3$; and its other compounds be represented by corresponding expressions, as shown in the left-hand column of the following Table, instead of by the heretofore-used more complex expressions shown in the right-hand column :—

Al 27·5.			Al 13·75.
$Al\,Cl^3$	....	Chloride	$Al^2\,Cl^3$
$Na\ Al\,Cl^4$	....	Sodio-chloride	$Na\ Al^2\,Cl^4$
$Na^3\,Al\,F^6$	....	Cryolite	$Na^3\,Al^2\,F^6$
$Na^3\,Al\,O^3$	....	Aluminate	$Na^3\,Al^2\,O^3$
$H\ \ Al\,O^2$	....	Diaspore	$H\ \ Al^2\,O^2$
$K\ \ Al\,S^2\,O^4$	....	Alum	$K\ \ Al^2\,S^2\,O^4$
$K\ \ Al\,Si^3\,O^8$	....	Felspar	$K\ \ Al^2\,Si^3\,O^8$
$P\ \ Al\,O^4$	....	Phosphate	$P\ \ Al^2\,O^4$

But the quantity of aluminium contained in two volumes of its gaseous chloride was found by Deville to be 55 parts instead of 27·5 parts, while the quantity of chlorine was found to be *six* times 35·5 parts instead of *three* times 35·5 parts. Hence, relying exclusively upon molecular volume, the atomic weight of aluminium would be 55, and the formula of chloride of aluminium $Al\,Cl^6$. This conclusion, however, is inadmissible for several reasons, and chiefly because it would make the atomic proportion of aluminium possess a specific heat twice as great as that belonging to the atomic proportion of any other metal.

To evade this difficulty, some chemists have proposed to accord to the molecule of aluminic chloride the formula $Al^2 Cl^6$, whereby an indivisible proportion of metal would be habitually represented by a divisible symbol; for it is agreed on all hands that the proportion of aluminium contained in the molecule of aluminic chloride is the smallest proportion of aluminium found in any aluminic compound whatsoever; that it is incapable of experimental division by any process whatsoever; and consequently that, so far as our present knowledge goes, it is an indivisible or atomic proportion.

Now there are undoubtedly certain bodies, elementary and compound, of which the ascertained vapour-densities, and consequent volumes, no matter how accounted for, are, as a mere matter of experiment, discordant with the chemical analogies of the respective bodies; but in most instances these anomalous results are rendered unimportant by other determinations of vapour-density, either of the same bodies raised to higher temperatures, or of associated bodies having a more decided volatility. Hence arises the question whether the ascertained volume of aluminic chloride, which is discordant with the specific heat of aluminium, may not be anomalous in a similar manner, and whether the anomaly may not be corrected by an examination of other more volatile aluminic compounds.

The methide and ethide of aluminium recently obtained by Mr. Buckton and the speaker are, so to speak, varieties of aluminic chloride in which the chlorine has been replaced by methyle and ethyle, and are at the same time far more volatile and manageable than the typical chloride. Now it has been found that two gaseous volumes of the methide and ethide of aluminium contain only 27·5 parts of aluminium, united with three atomic proportions of methyle and ethyle; and accordingly their molecules have to be expressed by the formulæ $AlMe^3$ and $AlEt^3$ respectively. In other words, the normal results obtained with the methide and ethide correct the anomalous result obtained with the chloride, and confirm the atomic weight and molecular formulæ deducible from the specific heat of aluminium.

That the ascertained vapour-density of aluminic chloride is really anomalous receives a further corroboration from the behaviour of aluminic methide itself. At 220° and all superior temperatures the vapour-density of this compound shows that two volumes of its vapour contain 27·5 parts of aluminium and *three* times 15 parts of methyle; but at 130° its vapour-density, corrected for alteration of temperature, becomes very nearly doubled, or, in other words, two volumes of its vapour contain very nearly 55 parts of aluminium and *six* times 15 parts of methyle. According, however, to the well-known rule, based on the separate researches of Cahours and Deville, the molecular formula of a body must be calculated from its permanent or ultimate, and not from its variable or initial vapour-density; whence the high vapour-density of aluminium-methide at 130° does not at all interfere with our attributing to its molecule the formula $AlMe^3$, deducible from its vapour-density at 220° and upwards, and harmonizing with the specific heat of metallic aluminium.

Aluminium-ethide and methide occur as colourless liquids. The ethide boils at 194°, and does not freeze at −18°. The methide boils at 130°, and solidifies at a little above 0° into a beautiful crystalline mass. Both liquids take fire on exposure to the air, and explode violently by contact with water. They are produced from mercuric ethide and methide respectively by heating these compounds for some hours in a water-bath, with excess of aluminium clippings. This process was obviously suggested by Frankland and Duppa's new reaction for making zinc-ethide, methide, amylide, &c.

$$Al^2 + 3HgEt^2 = Hg^3 + 2AlEt^3.$$

ROYAL SOCIETY.

[Continued from p. 239.]

January 26, 1865.—Major-General Sabine, President, in the Chair.

The following communications were read :—

"On the Spectrum of the Great Nebula in the Sword-handle of Orion." By William Huggins, F.R.A.S.

In a paper recently presented to the Royal Society*, I gave the results of the application of prismatic analysis to some of the objects in the heavens known as nebulæ. Eight of the nebulæ examined gave a spectrum indicating gaseity, and, of these, six belong to the class of small and comparatively bright objects which it is convenient to distinguish still by the name of planetary. These nebulæ present little indication of probable resolvability into discrete points, even with the greatest optical power which has yet been brought to bear upon them.

The other two nebulæ which gave a spectrum indicative of matter in the gaseous form, are 57 M, the annular nebula in Lyra, and 27 M, the Dumbbell nebula. The results of the examination of these nebulæ with telescopes of great power must probably be regarded as in favour of their consisting of clustering stars. It was therefore of importance to determine, by the observation of other objects, whether any nebulæ which have been *certainly resolved* into stars give a spectrum which shows the source of light to be glowing gas. With this purpose in view I submitted the light of the following easily resolved clusters to spectrum analysis.

"4670. 2120 h. 15 M. Very bright cluster; well resolved" †.

"4678, 2125 h. 2 M. Bright cluster, well resolved."

Both these clusters gave a continuous spectrum.

I then examined the Great Nebula in the Sword-handle of Orion. The results of telescopic observation on this nebula ‡ seem to show

* On the Spectra of some of the Nebulæ, Phil. Trans. 1864, p. 437.

† The numbers and descriptions are from Sir John Herschel's Catalogue, Phil. Trans. 1864, part 1.

‡ "The general aspect of the less luminous and cirrous portion is simply nebulous and irresolvable; but the brighter portion immediately adjacent to the trapezium forming the square front of the head, is shown with the 18-inch reflector broken up into masses, whose mottled and curdling light evidently

that it is suitable for observation as a crucial test of the correctness of the usually received opinion that the resolution of a nebula into bright stellar points is a certain and trustworthy indication that the nebula consists of discrete stars after the order of those which are bright to us. Would the brighter portions of the nebula adjacent to the trapezium, which have been resolved into stars, present the same spectrum as the fainter and outlying portions? In the brighter parts, would the existence of closely aggregated stars be revealed to us by a continuous spectrum, in addition to that of the true gaseous matter?

The telescope and spectrum apparatus employed were those of which a description was given in my paper already referred to.

The light from the brightest parts of the nebula near the trapezium was resolved by the prisms into three bright lines, in all respects similar to those of the gaseous nebulæ, and which are described in my former paper.

These three lines, indicative of gaseity, appeared (when the slit of the apparatus was made narrow) very sharply defined and free from nebulosity; the intervals between the lines were quite dark.

When either of the four bright stars, α, β, γ, δ Trapezii was brought upon the slit, a continuous spectrum of considerable brightness, and nearly linear (the cylindrical lens of the apparatus having been removed), was seen, together with the bright lines of the nebula, which were of considerable length, corresponding to the length of the opening of the slit. The fifth star γ' and the sixth α' are seen in the telescope, but the spectra of these are too faint for observation.

The positions in the spectra of α, β, γ, δ Trapezii, which correspond to the positions in the spectrum of the three bright lines of the nebula, were carefully examined, but in no one of them were dark lines of absorption detected.

The part of the continuous spectra of the stars α, β, γ, near the position in the spectrum of the brightest of the bright lines of the nebula, appeared on a simultaneous comparison to be more brilliant than the line of the nebula, but in the case of γ the difference in brightness was not great. The corresponding part of δ was perhaps fainter. In consequence of this small difference of brilliancy, the bright lines of the adjacent nebula appeared to cross the continuous spectra of γ and δ Trapezii.

Other portions of the nebula were then brought successively upon the slit; but throughout the whole of those portions of the nebula which are sufficiently bright for this method of observation the spectrum remained unchanged, and consisted of the three bright

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indicates, by a sort of granular texture, its consisting of stars, and when examined under the great light of Lord Rosse's reflector, or the exquisite defining power of the great achromatic at Cambridge, U. S., is evidently perceived to consist of clustering stars. There can therefore be little doubt as to the whole consisting of stars too minute to be discerned individually even with these powerful aids, but which become visible as points of light when closely adjacent in the more crowded parts . . . ."—Sir John Herschel, 'Outlines of Astronomy,' 7th edition, pp. 651, 652.

lines only. The whole of this Great Nebula, as far as it lies within the power of my instrument, emits light which is identical in its characters; the light from one part differs from the light of another in intensity alone.

The clustering stars of which, according to Lord Rosse and Professor Bond, the brighter portions of this nebula consist, cannot be supposed to be invisible in the spectrum apparatus because of their faintness, an opinion which is probably correct of the minute and widely separated stars seen in the Dumb-bell nebula, and to which reference was made in my former paper. The evidence afforded by the largest telescopes appears to be that the brighter parts of the nebula in Orion consist of a "mass of stars;" the whole, or the greater part of the light from this part of the nebula, must therefore be regarded as the united radiation of these numerous stellar points. Now it is this light which, when analyzed by the prism, reveals to us its gaseous source, and the bright lines indicative of gaseity are free from any trace of a continuous spectrum, such as that exhibited by all the brighter stars which we have examined.

The conclusion is obvious, that the detection in a nebula of minute closely associated points of light, which has hitherto been considered as a certain indication of a stellar constitution, can no longer be accepted as a trustworthy proof that the object consists of true stars. These luminous points, in some nebulæ at least, must be regarded as themselves gaseous bodies, denser portions, probably, of the great nebulous mass, since they exhibit a constitution which is identical with the fainter and outlying parts which have not been resolved. These nebulæ are shown by the prism to be enormous gaseous systems; and the conjecture appears probable that their apparent permanence of general form is maintained by the continual motions of these denser portions which the telescope reveals as lucid points.

The opinions which have been entertained of the enormous distances of the nebulæ, since these have been founded upon the supposed extent of remoteness at which stars of considerable brightness would cease to be separately visible in our telescope, must now be given up in reference at least to those of the nebulæ the matter of which has been established to be gaseous.

It is much to be desired that *proper motion* should be sought for in those of the nebulæ which are suitable for this purpose; indications of parallax might possibly be detected in some, if any nebulæ could be found that would admit of this observation.

If this view of the greater nearness to us of the gaseous nebulæ be accepted, the magnitudes of the separate luminous masses which the telescope reveals as minute points, and the actual intervals existing between them, would be far less enormous than we should have to suppose them to be on the ordinary hypothesis.

It is worthy of consideration that all the nebulæ which present a gaseous spectrum exhibit the *same three bright lines*; in one case only, 18 H.IV., was a fourth line seen. If we suppose the gaseous substance of these objects to represent the "nebulous fluid" out

of which, according to the hypothesis of Sir Wm. Herschel, stars are to be elaborated by subsidence and condensation, we should expect a gaseous spectrum in which the groups of bright lines were as numerous as the dark lines due to absorption which are found in the spectra of the stars. Moreover, if the improbable supposition be entertained, that the three bright lines indicate matter in its most elementary forms, still we should expect to find in some of the nebulæ, or in some parts of them, a more advanced state towards the formation of a number of separate bodies, such as exist in our sun and in the stars; and such an advance in the process of formation into stars would have been indicated by a more complex spectrum.

My observations, as far as they extend at present, seem to be in favour of the opinion that the nebulæ which give a gaseous spectrum, are systems possessing a structure, and a purpose in relation to the universe, altogether distinct and of another order from the great group of cosmical bodies to which our sun and the fixed stars belong.

The nebulous star ι Orionis was examined, but no peculiarity could be detected in its continuous spectrum*.

"Further Observations on the Planet Mars." By John Phillips, M.A., LL.D., F.R.S. F.G.S.

The return of Mars to his periodical opposition with the sun has enabled me to offer a few observations on this planet, in addition to those which on a former occasion I had the honour to present to the Society†. Among the subjects then suggested for consideration was the permanence of the main features of light and shade which had been recognized by many observers. Another question requiring attention referred to the fogginess or seeming cloudiness of the planet, also noticed by many observers, some of whom represented what might be thought effects of currents in the atmosphere round him. Again, it was a matter for further research whether the colours of what we suppose to be land and sea (the reddish hue of the land, and the grey aspect of the sea) were capable of explanation by any peculiarity of the soil or atmosphere, and whether, from the phenomena of snows visible about the poles and elsewhere, the climate of Mars could be estimated on trustworthy grounds.

My observations are too few to furnish answers for all these questions; but I have something to say in reply to some of them, though the distance of Mars from the Earth during the late opposition was too great to allow of such close scrutiny as in 1862.

First, then, in respect of the permanence of the main features of the planet. I submit several drawings‡ made between the 14th

* Admiral Smyth appears to have always maintained that the results of telescopic observation on the nebulæ were insufficient to support the opinion that all these objects were probably of stellar constitution. See his 'Cycle of Celestial Objects,' vol. i. p. 316; and his 'Speculum Hartwellianum,' pp. 111–114.

† Phil. Mag. S. 4. vol. xxvi. p. 312.

‡ Preserved for reference in the Archives; an equatorial projection is given in Plate II.

November and 13th December (both inclusive), the dates being marked on each, for comparison with others made in 1862, partly by Mr. Lockyer, partly by myself; from which it will immediately appear that no appreciable change has occurred in the main outlines of land and sea, in the longitudes observed. A certain fogginess has been noticed, especially on the 18th and 20th November, such as does not commonly occur with Jupiter or Saturn; but it seemed to be due to no essential circumstance of the planet, for it grew less and less as the observation approached the meridian.

The colour of the larger masses of land is the same as formerly observed, but fainter from distance; and the sea is grey and shadowy, but without the very distinct greenish hue which was noticed in 1862. Finally, the snows round the south pole appeared much less extensive than in 1862, and were not really observable with distinctness except on a few evenings. Snowy surfaces, scarcely more defined, but much more extensive, were observed in parts of the northern regions, not immediately encircling the pole (which was invisible), but in two principal and separate tracts estimated to reach 40° or 45° from the pole. On one occasion (30th November) two practised observers (Mr. Luff and Mr. Bloxidge) noticed with me one of these gleaming masses of snow, very distinct—so much so, that, as happened with the south polar snow in 1862, it seemed to project beyond the circular outline—an optical effect, no doubt, and due to the bright irradiation. This white mass reached to about 40° or 45° from the pole, in the meridian of 30° on my globe of Mars. Another mass was noticed on the 14th and 18th November, in long. 225°, and extending to lat. 50°. In each case the masses reached the visible limb.

The small extent of the snow visible at the further pole may be truly the effect of the position of the planet. If we remember that on this occasion the axis of Mars was nearly (within about 6° or 8°) at right angles to the line of sight, while in 1862 it was oblique (about 26°), we shall perceive that though the snow about the south pole were really as extensive in 1864 as it appeared to be in 1862, it could not possibly appear even nearly so large, and in fact could barely be seen (as it was) under the very small angle which it would subtend on the limb. There may, however, have been really less snow round the south pole, in consequence of the longer action of the summer heat on Mars in 1864 than in 1862.

The ruddy tint of the surface of the broad tracts of land is so constantly observed in these parts as to claim to be regarded as characteristic of some peculiarity in them—some special kind of terrestrial substance for example *. On the other hand, the tint is so much

* "In this planet we discern, with perfect distinctness, the outlines of what may be continents and seas. Of these, the former are distinguished by that ruddy colour which characterizes the light of this planet (which always appears red and fiery), and indicates, no doubt, an ochrey tinge in the general soil, like what the red sandstone districts on the earth may possibly offer to the inhabitants of Mars, only more decided. Contrasted with this (by a general law in optics) the seas, as we may call them, appear greenish."—Herschel's Astronomy (ed. 1833), p. 279.

like that of our evening clouds as to suggest the probability of its being due to the deep atmospheric zone which has been often ascribed to this planet, though perhaps, until of late years, on insufficient grounds*. On this head spectral analysis will probably enlighten us. If, however, there be such a deep covering of atmosphere, it might explain some facts regarding the climate which otherwise appear unaccountable. Some considerable amount of vaporous atmosphere there must be, to give origin to the beds of snow which alternately invest and desert the opposite poles, if indeed either pole be ever quite free from snow.

In different Martial years the extent of the snow appears nearly the same under nearly similar conditions. Compare, for instance, Herschel's drawing for August 16, 1830†, with my sketch for September 27, 1862‡, and that now presented for November 20, 1864.

Snows appear to have been observed in mass as far from the south pole as lat. 40°. This occurred in April 1856, according to a drawing by Mr. De la Rue: snow in lat. 50° or perhaps 45° North is the result of my observations during this late opposition. Assuming this to be the geographical limit of the freezing mean winter temperature, we see at once that it differs but little from that of the earth, on which the isothermal line of 32° varies, according to local peculiarities, from the latitude of 40° to that of 60°. If the snows on the land of Mars be compared with those on the northern tracts of Asia and America, they will be found not to extend further. And as the snows, if they do not actually disappear, are reduced to small areas about either pole in *its* warm season, thus showing the mean summer temperature there to be not less than 32°, this confirms the general impression that the variations of the climate of Mars are comprised within nearly the same thermic limits as those of the earth. In all the broad belt of 30° or 40° from the equator, the temperature seems to be such as always to allow of evaporation; between that limit and the pole, snows gather and disperse according to the season of the year, while for about 8° or 10° more or less round the pole, the icy circle seems to be perennial.

The relative mean distances from the Sun of Mars and the earth being taken at 100 and 152, the relative solar influence must be on Mars 100 to 231 on the earth; so that the surface of the more distant planet might rather be expected to have shown signs of being fixed in perpetual frost, than to have a genial temperature of 40° to 50°, if not 50° to 60°, as the earth has, taken on the whole. How is this to be accounted for? Of two conceivable influences which may be appealed to, viz. very high interior heat of the planet, and some peculiarity of atmosphere, we may, while allowing some value to each, without hesitation adopt the latter as the more immediate and effective.

To trace the effects in detail must be impracticable; but in the general we may remark that as a diminution of the mass of vaporous

* "It has been surmised to have a very extensive atmosphere, but on no sufficient or even plausible grounds."—Herschel's Astronomy (ed. 1833), p. 279, note.

† Treatise on Astronomy (ed. 1833), pl. 1. ‡ Phil. Mag. S. 4. vol. xxvi. p. 314.

atmosphere round the earth would greatly exaggerate the difference of daily and nightly, and of winter and summer temperature, so the contrary effect would follow from an augmentation of it. Applying this to Mars, we shall see that his extensive atmosphere would reduce the range of summer and winter, and of daily and nightly temperature. It would, moreover, augment the mean temperature by the peculiar action of such an atmosphere, which, while readily giving passage to the solar rays, would resist the return of dark heat-rays from the terrestrial surface, and prevent their wasteful emission into space *. This effect obtains now on the earth, which is rendered warmer, as well as more equable in temperature, by the atmosphere than it would be without it. It is conceivable that it may obtain upon Mars to a greater degree, even without supposing the atmosphere to be materially different in its nature from that round the earth, or the surface of Mars to have any specially favourable or exceptional characters for the absorption and radiation of heat. It seems, however, requisite to suppose a greater communication of heat from the interior of the planet; for otherwise the additional vapour, to which the warming effect is in the main to be ascribed, could not probably be supported in the atmosphere. On the whole we may, perhaps, be allowed to believe that Mars is habitable.

Here, so far as direct observations upon the aspect of Mars are available, we may pause. The researches of the Radcliffe Observer, lately in Oxford, and formerly at Greenwich, have, however, brought into view a peculiarity in the constitution of this planet which deserves special notice. Its figure is spheroidal, as might be expected from the general laws of planetary form; but it is spheroidal in so high a degree as to be quite exceptional in this respect. Computing by the known rotation-velocity, and the admitted measures and mass of Mars, its ellipticity should be about $\frac{1}{300}$. Mr. Main's observations with the splendid Oxford Heliometer give as the most probable result, the large fraction of $\frac{1}{87\cdot59}$ for 1862. This excellent astronomer has continued his observations during the late opposition. My own attempts to obtain the ellipticity with the micrometer eyepiece reading to 0″·2 of arc failed to give satisfactory measures. The ellipticity, indeed, seemed to be small, and was merely observable, not really measurable or even to be approximately estimated by the help of this apparatus.

GEOLOGICAL SOCIETY.

[Continued from p. 241.]

January 11, 1865.—Sir R. I. Murchison, Vice-President, in the Chair.

The following communications were read:—

1. "On the Lias Outliers at Knowle and Wootton Waven in South Warwickshire." By the Rev. P. B. Brodie, M.A., F.G.S.

The author gave a description of the Liassic outliers at Knowle

* Life on the Earth, 1860, p. 163–65. Tyndall's Researches, Proceedings of the Royal Society, February 1861.

and Wootton in South Warwickshire. At Knowle, eleven miles S.E. of Birmingham, the Lower Lias is represented by limestone and shales containing *Ammonites planorbis*, Saurian remains, *Ostrea*, *Modiola*, &c.; below these beds with *Ammonites planorbis*, dark shales were seen resting on the New Red Marl; amongst the shales occurs a micaceous sandstone with *Pullastra arenicola*, which elsewhere prevails low down in the series, in close connexion with the bone-bed. The greater outlier at Wootton Park, near Henley, exhibited more clearly the succession of the deposits, from the beds with *Pecten Valoniensis* up to the limestone with *Lima gigantea*, &c.

2. "On the History of the last Geological Changes in Scotland." By T. F. Jamieson, Esq., F.G.S.

The history of the last geological changes in Scotland, as given in this paper, was divided into three periods, namely, the Preglacial, the Glacial, and the Postglacial.

The absence of the later Tertiary strata from Scotland leaves the history of the Preglacial period very obscure; but the author considered it in some degree represented by some thick masses of sand and gravel (apparently equivalent to the Red Crag of England) on the coast of Aberdeenshire; and he stated that there were indications of the Mammoth having inhabited Scotland during this period.

The Glacial period was divided into three successive portions, namely, (1) the Period of Land-ice, during which the rocky surface was worn, scratched, and striated, and the boulder-earth, or glacier-mud, was formed; (2) the Period of Depression, in which the glacier-marine beds were formed; and (3) the Period of the Emergence of the land to which belong the valley-gravels and moraines, and during which the final retreat of the glaciers took place.

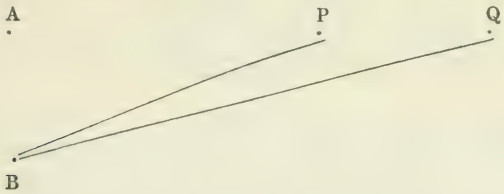
To the Postglacial period Mr. Jamieson referred that of the formation of the submarine forest-beds, which he considered was succeeded by a second period of depression, and this again by the elevation of the land to its present position. It is in the old estuary beds and beaches formed during the Second Period of Depression that the author finds the first traces of Man in Scotland, while the Shell-mounds with chipped flints he referred to the same epoch as the blown sand and beds of peat, namely to the most recent period, during which the land was raised to its present level.

Mr. Jamieson described in great detail the deposits representing each of these periods, and concluded his paper with lists of shells from the different beds, showing the percentage of the species that are now found in the British, Southern, Arctic, N.E. American, and North Pacific regions.

XLV. *Intelligence and Miscellaneous Articles.*

SIMPLE MODE OF DETERMINING THE POSITION OF AN OPTIC IMAGE. BY A. KRÖNIG.

ON looking with one eye at two points which form no, or at all events a very small visual angle with one another, it is easily determined whether one of the points is more distant, and which it is. It is merely necessary, while continuing to look at the two points, to move the eye at right angles to the direction of its axis, and to observe the simultaneous apparent motion of the two points. That point which moves in the same direction with the eye is the most distant. In the annexed figure, let A be the eye which looks at the



two points P and Q. Let A, P, and Q be in the same right line. The eye is now moved from A to B. The two points P and Q thereby diverge from one another by the angle P B Q, and Q appears to move in the same direction as the eye (to the right, for instance), but P in the opposite direction. Inversely, when P appears to move in the same direction as the eye, it follows that P is more distant from the eye than Q.

Imagine now an optical apparatus which projects an objective picture of any object. If the image is not sufficiently bright to be caught upon a screen, the law (that of two unequally distant points the further moves in the same direction as the eye) furnishes an easy means of finding the position of an image. The eye is so placed that it views the image. A needle-point is then brought by the hand, or still better by means of a fixed stand, into such a position that the point covers any part of the picture. The head is then moved somewhat on one side. If now either the needle-point or the point of the picture moves in the same direction as the eye, this point is further removed from the eye than the other, which moves in the opposite direction. By successively shifting the needle-point, the position is readily found in which, when the eye is moved, the needle-point coincides with the point of the picture considered. This is the position of the image. If the needle-point, after it has been approached to the reflecting or refracting apparatus till it touches, still appears to move in an opposite direction to the eye, the image is subjective, and not objective.

In order to extend the same process to subjective images, it would first be necessary to prove experimentally and directly that the image produced by a plane mirror actually occupies the position hitherto

assumed. For this purpose two equal threads stretched vertically by weights might be used, and a plane mirror without edge be so adjusted that the reflected piece of the one thread falls in a straight line with the directly seen parts of the other in each position of the eye.

After the position of the image produced by a plane mirror has thus been fixed, the discovery of the position of other subjective pictures will be possible by means of a transparent and at the same time reflecting plane-parallel plate on which a luminous point is reflected. The plate is to be so arranged that the reflected picture of the luminous point covers a point of the image in question. An assistant must move the luminous point until the reflected and refracted image no longer move towards each other in any motion of the eye. The directly measured distance of the reflected point from the plane-parallel plate is equal to the required distance of the subjective image produced by the optical apparatus from the same plate. —Poggendorff's *Annalen*, vol. cxxiii. p. 655.

ON A SIMPLIFIED METHOD OF EXTRACTING INDIUM FROM THE FREIBERG ZINCBLENDES. BY M. WESELSKY.

The roasted and levigated blendes are treated with a mixture of ten parts of hydrochloric and one of nitric acids; the solution, separated from silica and the liberated sulphur, is greatly diluted with water, and carbonate of soda added until a precipitate first begins to form. The solution is boiled, hyposulphite of soda being added until no more sulphurous acid escapes, and the precipitate, which at first is yellowish and flocculent, has become black, when it readily settles down. The solution contains, besides all the iron and zinc, small quantities of arsenic and copper, and also part of the indium. The black precipitate consists of the sulphur-compounds of arsenic, lead, copper, &c., and contains the rest of the indium. Without removing it, freshly-precipitated carbonate of baryta in excess is added to the liquid when it is cold, and the whole allowed to stand for twelve hours. The precipitate, which, besides the above sulphides, contains all the indium and the excess of carbonate of baryta, is well washed, the air being excluded, and is then treated with dilute hydrochloric acid. In this way the carbonate of baryta and the indium are dissolved. To remove a small quantity of sulphides which pass into solution, sulphuretted hydrogen is passed into the acid solution; and baryta is removed by sulphuric acid. Oxide of indium is separated from any possibly adhering oxides of iron or zinc by means of carbonate of baryta.

From experiments with which M. Weselsky is at present occupied, it appears that, under suitable circumstances, indium may be completely precipitated by hyposulphite of soda, by which the application of carbonate of baryta is quite avoided.—*Bulletin der Akademie in Wien*, vol. vii. p. 1869.

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[FOURTH SERIES.]

MAY 1865.

XLVI. *Supplementary Considerations relating to the Undulatory Theory of Light.* By Professor CHALLIS, *F.R.S., F.R.A.S.**

ON a review of the arguments by which I have now for a long time maintained that the phenomena of light are referable to the vibrations and pressures, as mathematically determined, of a continuous elastic fluid the pressure of which varies proportionally to its density, I have found that there are certain points of the reasoning which require rectification or confirmation. To discuss these points is the object of the present communication.

(1) By pure reasoning, founded on admitted principles, I have ascertained that the vibratory motion of the supposed elastic fluid is *composite* independently of particular modes of disturbance, and that each component consists of vibrations partly parallel and partly transverse to an *axis*. The former of these results is at once applicable in accounting for the *composition* of light as indicated by prismatic analysis, and the other in the explanation of facts of *polarization*. As the reasoning also showed, independently of arbitrary disturbances, that for small vibrations $udx+vdy+wdz$ is an exact differential, the motion relative, as above stated, to an axis is analytically expressed by the equation

$$(d.f\phi)=udx+vdy+wdz,$$

f being a function of x and y only, ϕ a function of z and t only, and the axis of the motion coinciding with the axis of z. In fact this equation gives

$$u=\phi\frac{df}{dx},\quad v=\phi\frac{df}{dy},\quad w=f\frac{d\phi}{dz};$$

* Communicated by the Author.

and we may assume f to be such that, where $x=0$ and $y=0$,

$$f=1,\quad \frac{df}{dx}=0,\quad \frac{df}{dy}=0.$$

The details of the reasoning here referred to are given in the proof of Proposition X. contained in an Article on the Principles of Hydrodynamics in the Philosophical Magazine for December 1852. In the same Article are investigated exact expressions for the functions f and ϕ, and the rate of propagation of the motion along the axis is found to be $a\sqrt{1+\frac{e\lambda^2}{\pi^2}}$, a and λ having the usual significations, and e being a constant such that, where $x=0$ and $y=0$,

$$\frac{d^2f}{dx^2}+\frac{d^2f}{dy^2}+4ef=0.$$

As e is necessarily a positive quantity, it follows that the rate of propagation, *as determined by hydrodynamics*, is greater than a. It is further evident, putting κa for the rate, that if κ be a numerical constant, its value should be determinable exclusively on hydrodynamical principles. This is what I have attempted to do in a communication to the Philosophical Magazine for February 1853; but having recently discovered that the mathematical reasoning there given requires correction, I propose now to enter upon the discussion of this point.

The determination of the constant e depends on the integration of the equation

$$\frac{d^2f}{dr^2}+\frac{df}{rdr}+4ef=0,$$

r being any distance from the axis of motion. The integral is not obtainable in a finite form, but it may readily be shown that the following series for f satisfies it, viz.

$$f=1-er^2+\frac{e^2r^4}{1^2.2^2}-\frac{e^3r^6}{1^2.2^2.3^2}+\&c.$$

For finding e it is required to ascertain the large values of r that make f vanish. This problem is solved by Sir W. Hamilton in a memoir on Fluctuating Functions in the Transactions of the Royal Irish Academy (vol. xix. p. 313), and by Professor Stokes in the Transactions of the Cambridge Philosophical Society (vol. ix. part 1. p. 182). I hesitated to accept the equation (52) in the latter memoir, because it contains quantities R and S representing series that are convergent for a certain number of terms and then become divergent, which yet are employed as if they were wholly convergent. Whatever be the answer to this

objection, it is certain that the equation obtained by Professor Stokes for large values of r, viz.

$$f=(2\pi r\sqrt{e})^{-\frac{1}{2}}(\cos 2\sqrt{e}r+\sin 2\sqrt{e}r),$$

approaches the true integral in proportion as r is larger. In fact this equation is the exact integral of the differential equation

$$\frac{d^2f}{dr^2}+\frac{df}{rdr}+4ef=\frac{f}{r^2},$$

which evidently differs less from the foregoing differential equation as r is larger. Hence it appears that the consecutive large values of r which cause f to vanish, increase by the common difference $\frac{\pi}{2\sqrt{e}}$. By peculiar reasoning applied to the infinite roots of the equation $f=0$ (Phil. Mag. for February 1853), I found the common difference to be ultimately $\frac{1}{\sqrt{e}}$. But from the preceding argument it must be concluded that that reasoning is not legitimate, and that some error is involved in the treatment of the infinite roots. It will therefore be necessary to determine the rate of propagation by a new investigation, employing for the purpose the above expression for f. This I proceed to do by a course of reasoning analogous to that which was followed in the previous investigation.

In the article already cited, containing Proposition X., the following equations to the first approximation are obtained:—

$$\phi=m\cos\frac{2\pi}{\lambda}(z-\kappa at+c),\quad \kappa=\sqrt{1+\frac{e\lambda^2}{\pi^2}},\quad a^2\sigma+f\frac{d\phi}{dt}=0$$

Also if w and ω be respectively the velocities parallel and transverse to the axis of z, we have $w=f\frac{d\phi}{dz}$ and $\omega=\phi\frac{df}{dr}$. The foregoing expression for f may be put under the form

$$(4\pi r\sqrt{e})^{-\frac{1}{2}}\cos\left(2\sqrt{e}r-\frac{\pi}{4}\right);$$

and as r is assumed to be very large, it may be supposed to have the constant value r_0 outside the cosine, and the general value r_0+h under the cosine, h being always very small compared to r_0. Then putting, for brevity, f_0 for the constant coefficient, and c' for $2\sqrt{e}r_0-\frac{\pi}{4}$, we shall have

$$f=f_0\cos(2\sqrt{e}h+c'),\quad \frac{df}{dr}=-2\sqrt{e}f_0\sin(2\sqrt{e}h+c').$$

Consequently

$$w = -\frac{2\pi m f_0}{\lambda}\cos(2\sqrt{e}h + c')\sin\frac{2\pi}{\lambda}(z - \kappa a t + c),$$

$$\omega = -2m\sqrt{e}f_0\sin(2\sqrt{e}h + c')\cos\frac{2\pi}{\lambda}(z - \kappa a t + c),$$

$$\sigma = -\frac{2\pi m \kappa f_0}{\lambda a}\cos(2\sqrt{e}h + c')\sin\frac{2\pi}{\lambda}(z - \kappa a t + c).$$

Hence it follows that $w = \frac{a\sigma}{\kappa}$. Suppose now another series of waves, exactly equal to the first, to be propagated in the contrary direction; and let z_0 be the coordinate of a position at which the velocity parallel to the axis is constantly zero. Then if in general $z = z_0 + l$, and if w', ω', and σ' be respectively the resulting velocities and condensation, we shall have

$$w' = -\frac{2\pi m f_0}{\lambda}\cos(2\sqrt{e}h + c')\left(\sin\frac{2\pi}{\lambda}(l - \kappa a t) + \sin\frac{2\pi}{\lambda}(l + \kappa a t)\right),$$

$$\omega' = -2m\sqrt{e}f_0\sin(2\sqrt{e}h + c')\left(\cos\frac{2\pi}{\lambda}(l - \kappa a t) + \cos\frac{2\pi}{\lambda}(l + \kappa a t)\right),$$

$$\sigma' = -\frac{2\pi m k f_0}{\lambda a}\cos(2\sqrt{e}h + c')\left(\sin\frac{2\pi}{\lambda}(l - \kappa a t) - \sin\frac{2\pi}{\lambda}(l + \kappa a t)\right).$$

From the first and second of these equations it appears that

$$\frac{\omega'}{w'} = \frac{\sqrt{e}\lambda}{\pi}\cdot\tan(2\sqrt{e}h + c')\cot\frac{2\pi l}{\lambda}.$$

Let now the distance r_0 apply to positions at which the transverse velocity is always zero; and in order to get rid of the negative signs, and to avoid double signs, let $c' = (2n+1)\pi$. Then supposing h and l to represent very small *equal* distances from a point (r_0, z_0), where the velocity is constantly zero, we have ultimately

$$\frac{\omega'_0}{w'_0} = \frac{\sqrt{e}\lambda}{\pi}\cdot\frac{\tan 2\sqrt{e}h}{\tan\frac{2\pi l}{\lambda}} = \frac{e\lambda^2}{\pi^2}.$$

This result informs us that the changes of condensation produced by the flow of the fluid to or from any point of no velocity are due to the longitudinal and the transverse motions in a constant ratio. It hence follows that the changes of condensation at *any* given point of a single series of waves are due at each instant to the longitudinal and transverse motions in the same ratio. In

fact, from the foregoing values of w and ω, it will be seen that the ratio of $\frac{\delta\omega}{\delta h}$ to $\frac{\delta w}{\delta l}$ is that of e to $\frac{\pi^2}{\lambda}$. Hence if $\delta h = \delta l$, we have $\frac{\delta\omega}{\delta w} = \frac{e\lambda^2}{\pi^2}$, which is clearly the ratio in which the two velocities contribute to the changes of condensation.

By substituting $\frac{\omega'_0}{w'_0}$ for $\frac{e\lambda^2}{\pi^2}$ in the value of κ, we obtain for the velocity of propagation $a\sqrt{1+\frac{\omega'_0}{w'_0}}$; which shows that the excess of the velocity above the value a is caused by the transverse velocity, and that, because the changes of condensation are due to the transverse as well as the longitudinal velocity, they are more rapid than they would be if due to the latter alone, and the rate of propagation of a given state of density is consequently accelerated.

Reverting now to the expressions for ω' and σ', if $(2n+1)\pi$ be substituted for c', $\frac{2\pi}{\lambda'}$ for $2\sqrt{e}$, and κ' for $\frac{\kappa\lambda'}{\lambda}$, the following equations may be obtained:—

$$\omega' = \frac{2\pi m f_0}{\lambda'}\cos\frac{2\pi l}{\lambda}\left(\sin\frac{2\pi}{\lambda'}(h-\kappa' at)+\sin\frac{2\pi}{\lambda'}(h+\kappa' at)\right),$$

$$\sigma' = \frac{2\pi m\kappa' f_0}{\lambda' a}\cos\frac{2\pi l}{\lambda}\left(\sin\frac{2\pi}{\lambda'}(h-\kappa' at)-\sin\frac{2\pi}{\lambda'}(h+\kappa' at)\right).$$

At the same time

$$w' = \frac{2\pi m f_0}{\lambda}\cos\frac{2\pi h}{\lambda'}\left(\sin\frac{2\pi}{\lambda}(l-\kappa at)+\sin\frac{2\pi}{\lambda}(l+\kappa at)\right),$$

$$\sigma' = \frac{2\pi m\kappa f_0}{\lambda a}\cos\frac{2\pi h}{\lambda'}\left(\sin\frac{2\pi}{\lambda}(l-\kappa at)-\sin\frac{2\pi}{\lambda}(l+\kappa at)\right).$$

Hence the transverse motion and condensation may be represented by equations exactly analogous to those which represent the longitudinal motion and condensation, and the two motions are correlative to each other. If $\frac{\pi^2}{\lambda'^2}$ be substituted for e in the value of κ, we obtain $\kappa = \sqrt{1+\frac{\lambda^2}{\lambda'^2}}$. Also $\kappa' = \frac{\kappa\lambda'}{\lambda} = \sqrt{1+\frac{\lambda'^2}{\lambda^2}}$. Thus the velocities of propagation κa and $\kappa' a$ are each greater than a, because, as already explained, the transverse and longitudinal motions both contribute to the changes of condensation. But the *ratio* of these velocities is that of λ to λ', as evidently should be the case, since the propagations over these breadths occupy necessarily the same time.

There remains another consideration which must be brought to bear on the determination of the velocity of propagation. We found above the general relation $w=\frac{a\sigma}{\kappa}$ between the longitudinal velocity and condensation in a single series of waves. The analogous relation obtained by the process of reasoning that has been usually adopted in questions of this kind is $w=a\sigma$. But that process does not take into account that the total motion is composed of separate longitudinal and transverse motions relative to *axes*. The factor $\frac{1}{\kappa}$ is wholly due to the *lateral spreading* which accompanies the condensations and rarefactions propagated along and parallel to the axis of motion, which has the effect of diminishing the rate of change of the density in the direction of propagation, and thus making the effective elasticity of the fluid, *cæteris paribus*, less than the actual in the ratio of $\frac{a^2}{\kappa^2}$ to a^2. But just in the proportion in which the effective elasticity is caused by lateral spreading to be less than the actual in the direction of propagation, it must, by a reciprocal action, be made greater than the actual in the transverse direction, and accordingly be increased in the ratio of κ^2a^2 to a^2. Thus the ratio of the latter effective elasticity to the other is κ^4, and the ratio of the corresponding velocities of propagation is κ^2. Now we have proved that this ratio is $\frac{\lambda'}{\lambda}$. Hence, substituting in the expression for κ, we have

$$\kappa=\sqrt{1+\frac{1}{\kappa^4}},\text{ or }\kappa^6-\kappa^4=1.$$

Consequently the numerical value of κ^2 is obtained by the solution of a cubic equation which has one real positive root and two imaginary roots. The value of κ will be found to be 1·2106. Hence, taking $a=916{\cdot}322$ feet, the resulting velocity of propagation is 1109·3 feet. The value by observation, as given by Sir J. Herschel in the *Encyclopædia Metropolitana*, is 1089·7 feet. The difference 19·6 feet might be lessened in some degree by calculating the corrections of the observations for temperature according to Regnault's coefficient of expansion. But probably the principal part of the difference is due to the circumstance that the theoretical reasoning assumes the fluid to be *perfect*, and it may be that atmospheric air is not strictly such. It seems hardly to be accounted for that a course of reasoning involving considerations so various and peculiar as those which have been gone through above, should have conducted to a result differing

from observation by no larger amount, unless the principles of the reasoning are fundamentally correct. I may here state that the point of no velocity might have been taken on the axis of motion instead of being at a great distance from it, inasmuch as the motion contiguous to the axis may be supposed to consist of two equal sets of longitudinal and transverse motions; and each set might be treated independently of the other. By conducting the reasoning in this way I obtained the same results as by the other method.

If it be objected that when the effect of the development of heat on the rate of propagation is taken into account the mathematical result is contradicted by experiment, I reply as follows:—It is evident, from the mutual relation of the longitudinal and transverse motions above described, that we have had under consideration a case of *free* expansions and contractions due to successive generations and fillings up of a partial vacuum. Now it is admitted, I believe, that experiment has decided that in such a case there is no change of temperature. Consequently the rate of propagation remains unaffected. The case of development or absorption of heat when air is suddenly let into, or abstracted from, *closed* spaces, and when, in consequence, *work* is done, has no analogy to this. Upon the whole I seem entitled to conclude that I have at length succeeded in solving the difficult problem of determining mathematically the rate of propagation in a continuous elastic fluid. The results obtained are essential to the undulatory theory of polarization.

(2) I proceed, in the next place, to advert to a communication I made to the Philosophical Magazine for January 1857, entitled "On the Transmutation of Rays of Light." In the course of the article I have enumerated various inferences relating to phenomena of light, which had been deduced by means of the analysis I had applied to the undulatory theory; and to one of these, which is numbered (4) in the order of the series, I wish now to call attention. That deduction is expressed in the following terms:—"When the æther in motion suffers disturbance by encountering atoms actually or relatively at rest, and the original motion is a simple series of vibrations of the usual type, or is compounded of several such motions with parallel axes and different values of m, λ, and c, the result of the disturbance may in either case consist of an indefinite number of separate motions having their axes in various directions, and having values of m, λ, and c altogether different from the values of these quantities in the original motion." Further on I remark that "when the circumstances of the disturbance are as supposed in (4), light may produce new light, which may differ from the original light in intensity, colour composition, and direction of

propagation. This effect I have called a 'Transmutation of Rays;' and I beg it may be understood that in making use of these terms I mean only to express a result deduced from the mathematical theory." In writing the last sentence I had in mind the statement made by Professor Stokes, that change of refrangibility always took place from a greater to a less refrangibility. As there was nothing in my theory of Transmutation which pointed to such a limitation, and as the experimental evidence for it appeared to be only negative, I preferred stating the theoretical results in all their generality, without citing any experiments bearing upon them. But now that the experiments of Dr. Tyndall have shown that this law of transmutation applies to the less refrangible as well as to the more refrangible rays, and that there may be change from less to greater refrangibility, I feel at liberty to say that the theory is in complete accordance with these experimental results. I take this occasion to remark that the term "Transmutation of Rays," which has acquired special interest since Dr. Tyndall's experiments have shown that it expresses a law of nature, was originated by me, on purely theoretical grounds, in the communication here referred to, published more than eight years ago, and has since been adopted without any reference to its occurrence in that communication.

(3) In my Theory of the Composition of Colours, contained in the Philosophical Magazine for November 1856, I have endeavoured, under section (5), to give reasons for a distinction between "terrestrial light," that is, light which has been reflected, refracted, or generated by terrestrial substances, and direct solar light. I was induced to do this by the persistent assertion of experimenters that a composition of yellow and blue solar rays does not produce a green colour, whereas the composition of such rays emanating from yellow and blue terrestrial substances undoubtedly produces green. More recently, an experiment by Sir J. Herschel, described in the 'Proceedings of the Royal Society' (vol. x. No. 35, p. 82), has led me to infer that the distinction I sought to account for does not really exist. This experiment renders it very probable that in cases in which green is not perceived to result from a mixture of yellow and blue solar rays, the rays are of too great intensity for the eye to distinguish the colour. At least Sir J. Herschel found, after concentrating a solar spectrum by an achromatic lens, so as to bring the yellow, green, and blue spaces pretty close together, that, on *diminishing the intensity of the light*, the green appeared to be so diffused as to encroach greatly on the yellow and blue spaces. When making the experiment of covering white paper with alternating parallel spaces, not inconsiderable in breadth,

of yellow and blue colours made by chalk pencils, I constantly found that even when the eye was near enough to distinguish the spaces easily, the whole appeared to be suffused by a tinge of green. Now, although this diffusion in both kinds of experiment may be referable to the manner in which the organ of sight is acted upon by the rays, it proves not the less that a combination of yellow and blue has the same effect in producing green, whether the light come directly from the sun, or is what I called terrestrial light. For this reason I withdraw the distinction I endeavoured to establish between solar light and terrestrial light.

With reference to the same subject, I take this opportunity to state that I have made experiments for showing the effects of combining colours by means of revolving disks, the disks being divided into spaces covered alternately with the two colours to be compounded. The apparatus I used was professedly made according to directions contained in Professor Maxwell's paper on this subject, and among the different sets of colours was one which was intended to show that yellow and blue combined do not produce green. The result in this instance was certainly a dirty white; but according to my sight the blue and the yellow had scarcely any resemblance to prismatic blue and yellow. On substituting for them the very same chalk colours that I used in the above-mentioned experiment of parallel spaces, I found that the result was decidedly green. It may be that the colours I used were not pure colours; but the fact that one appeared blue and the other yellow was owing to the predominance of blue or yellow solar rays, and the predominant tint of the compound was determined accordingly.

For these reasons, drawn, it will be seen, in part from personal observations, I hold that sunlight and terrestrial light are not essentially different, and that, in accordance with the mathematical theory of the composition of colours given in the above-cited article, combinations of yellow and blue, with either kind of light, have the effect of producing green.

(4) It having been suggested to me to employ Ångström's values of λ, given in Poggendorff's *Annalen* for November 1864, for testing my Theory of the Dispersion of Light contained in the Supplementary Number of the Philosophical Magazine for December 1864, I have calculated as follows for this purpose. It was considered sufficiently accurate to obtain the values of λ for the rays C, D, F, and G from those for the rays B, E, and H, and the given values of μ by mere interpolation, and to regard the *differences* of the results deduced from the old and the new values of λ as the same that would have

been obtained by calculating strictly according to the theoretical formula (θ). According to this principle, it is only necessary to apply these differences to the values of λ previously calculated from Fraunhofer's data, to obtain the values that would be given by Ångström's data. These calculations having been gone through, the comparison, to the third place of decimals, of the observed and calculated values of λ for the two sets of data stands as follows :—

Ray.	λ by Fraunhofer.	Excess of calculation.		λ by Ångström.	Excess of calculation.	
		Flint-glass No. 13.	Oil of Cassia.		Flint-glass No. 13.	Oil of Cassia.
B	2·541	0·000	0·000	2·5397	0·000	0·000
C	2·422	+0·003	+0·006	2·4263	−0·002	+0·001
D	2·175	−0·001	−0·001	2·1786*	−0·003	−0·003
E	1·945	0·000	0·000	1·9484	0·000	0·000
F	1·794	+0·002	−0·003	1·7973	+0·003	−0·001
G	1·587	+0·005	−0·004	1·5923	+0·004	−0·003
H	1·464	0·000	0·000	1·4672	0·000	0·000

It appears from this comparison that the excesses of calculation are somewhat smaller with Ångström's values than with Fraunhofer's, especially in the case of oil of cassia, the more refractive substance.

With this communication I conclude the series of arguments by which I maintain that the Undulatory Theory of Light rests legitimately on no other than a hydrodynamical basis.

Cambridge, April 22, 1865.

XLVII. *On the Reversal of the Spectra of Metallic Vapours.* By H. G. Madan, *F.C.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, April 11, 1865.

MAY I be allowed to mention a simple and convenient method of illustrating one of the most important points in Bunsen and Kirchhoff's spectrum discoveries, viz. the reversal of the spectra of metallic vapours; the most familiar example of which is the reversal of the sodium-line D?

I have tried most of the various methods proposed for effecting this object, but none have appeared to me so easy and effective as the following.

* This value applies to the middle of the double line.

It consists simply in directing the spectroscope upon a fragment of sodium burning in oxygen gas. The incandescent metal gives, of course, a continuous spectrum; but the rays, in passing through the cooler atmosphere of sodium-vapour which surrounds the metallic nucleus, are selectively absorbed, and the dark double line D, or Na α, appears with great distinctness on the bright spectrum. As, however, the fragment of sodium is soon consumed, I have used an apparatus resembling that employed for making phosphoric anhydride, by which pellets of sodium may be added as often as required.

A moderate-sized deflagrating-jar is placed in a dish of sand. In it is suspended a shallow iron cup, and through the same cap which carries the latter is passed a short wide glass tube, so as to be directly over the centre of the cup. Through the sand and under the lower edge of the jar passes a bent glass tube, connected with a caoutchouc bag of oxygen, and serving to introduce a continuous slow stream of the gas to replace that consumed by the sodium. The spectroscope should first be adjusted as to position and focus by bringing it to bear on a candle placed on the opposite side of the jar, so that its flame may just be seen over the edge of the iron cup. Then, while the jar is filling with gas, the cup may be withdrawn, a pellet of sodium placed in it and heated over a spirit-lamp until it begins to burn, and lastly immersed in the jar. Fragments of sodium may be added as required through the glass tube, and will readily burn if the mass of soda in the cup be not allowed to cool below dark redness. The experiment may thus be carried on as long as desired; and, of course, two or more spectroscopes may be arranged round a single deflagrating jar.

I have not yet tried whether the spectrum thus produced can be thrown on a screen, the deflagrating-jar being enclosed in a Duboscq's lantern; but, from its brilliancy in the spectroscope, I have little doubt that it could be thus shown. The idea of this method occurred to me about a year ago, and I have shown it to many in Oxford; but it seemed so obvious an expedient that I thought it must have been already described. As, however, I have been unable to find any account of it, I venture now at any rate to bear my testimony to its efficacy.

The same method is of course available in the case of other volatile and oxidizable metals, as lithium, zinc, magnesium.

I remain,

Queen's College, Oxford. Yours, &c.,

H. G. Madan.

XLVIII. *On Phosphorite from Spain.* *By* David Forbes, *F.R.S., &c.**

THIS mineral was some time back forwarded from Spain for the purpose of being submitted to chemical examination; it was stated to occur in the district of Estramadura in Southern Spain in large quantity.

The specimens received, though in large blocks, consisted of the pure phosphorite, apparently uncontaminated and unaccompanied by any other minerals; the mineral itself was massive, and, on breaking, the fracture was uneven and earthy, disclosing at the same time a slightly radiating dendritic structure, but no trace whatever of crystallization. The colour of fresh fracture was chalk-white, but weathered surfaces frequently showed a dirty or rusty white colour externally. Lustre earthy; opake; streak and powder dead white.

Hardness about 4·5 on the scale, or immediately below apatite, by which it was scratched. The specific gravity was taken on two distinct specimens, and found to be 3·00 and 2·92 respectively at 60° F.

When the powdered mineral was heated over a lamp in the dark or thrown on to a nearly red-hot plate of iron, it quickly glowed, emitting an extremely beautiful and slightly greenish-yellow light, which passed off before the mineral attained a visibly red heat. As Mr. Phipson, in his treatise on Phosphorescence, states that minerals which, after having been heated, no longer emit the characteristic phosphorescent light upon reheating, again recover their fluorescent property upon being exposed to the sunlight, the experiment was tried several times and with various modifications, but failed even after many days' exposure to the sun's rays: this was also the case when the mineral, after heating once, was cooled in the dark and again reheated. In order also to see if the loss of moisture in the first heating had any relation to the phosphorescent property, another portion, after heating, was thrown into water and kept in the sun under water for many days; on being air-dried and reheated no signs of phosphorescence could be detected†.

In order to examine whether, as is the case with the mineral Gadolinite‡, the phenomenon of fluorescence, on heating, might

* Communicated by the Author.

† Mr. Phipson states that the fluor-apatites only are phosphorescent, and that this is not the case with the chlor-apatites. On examination, however, the fine crystallized true chlor-apatites from Krageroe in Norway emitted, on heating in the dark, a most distinct slightly greenish-yellow light, though not quite so bright as the above phosphorite.

‡ Gadolinite, when heated, glows suddenly and increases in density, being, after ignition, about 5 per cent. specifically heavier than before.

not be accompanied by a change in its specific gravity, two experiments were made upon fragments of the mineral, which, after having had their specific gravity determined, were heated to visible redness for some time, and, after cooling, a second determination of their specific gravity was made. In both cases a somewhat higher result was obtained after ignition than before, the exact numbers being as follows:—before ignition the specific gravities of the specimens were 3·00 and 2·92, but after ignition 3·12 and 2·98 respectively.

As the mineral was found to lose weight upon ignition (amounting to 1·44 per cent.), the specific gravity of the mineral after ignition was calculated from the weight of the mineral left behind after heating, and not from the weight of the phosphorite actually employed in the first instance, from which weight the specific gravity of the unignited mineral had been calculated.

It seems, however, not improbable that the increase of specific gravity may really be due to the mineral becoming less porous, or contracting, on losing the water (probably only hygroscopic), and not connected at all with the phenomenon of phosphorescence.

Before the blowpipe the mineral behaves as follows:—A thin fragment heated in the platinum-points remains unchanged, but at a very strong heat becomes rounded off at the edges, and, provided the mass operated upon is sufficiently small, ultimately fuses with great difficulty to a milk-white enamel. During this heating, in general no coloration of the outer flame can be perceived; but if the fragment be moistened in strong sulphuric acid and introduced into the point of the blue flame, a distinct bluish-green colour, quickly passing over, is seen at the moment of contact of the mineral with the flame.

In both open and closed tubes some little moisture is observed; and if a rather larger amount be heated in a bulbed tube and the evolved moisture tested, it will be found to feebly redden blue litmus paper, and give a somewhat yellow colour to Brazil-wood paper.

When heated with carbonate of soda it fuses with effervescence, part of the mass is absorbed by the charcoal, and a white residuum is left upon the surface.

Heated with borax-glass it dissolves to a transparent glass, somewhat yellow when hot, but colourless after cooling—a change no doubt owing to a small amount of iron present in the mineral itself. The glass, if sufficiently saturated, becomes milk-white; and if supersaturated with the mineral, becomes opake.

Phosphate of soda* dissolves the phosphorite with much more

* Instead of using the microcosmic salt or phosphate of soda and ammonia, I find it much better to use the anhydrous phosphate of soda left behind on heating microcosmic salt until all water and ammonia are evolved. The reactions in both cases are precisely similar.

facility and in larger quantity than borax-glass, and forms a clear colourless glass, which, however, if supersaturated, becomes a white enamel.

A portion of the mineral in the state of powder was placed in a small leaden capsule and drenched with strong sulphuric acid. Hydrofluoric acid was evolved upon gently heating the capsule, and etched deeply letters written through a film of wax varnish coating a glass plate with which the leaden capsule was covered.

The analysis of the mineral was conducted as follows:— 31·05 grains in powder were heated for some time to redness in a platinum crucible; upon cooling, the diminution in weight amounted to 0·45 gr., equivalent to 1·44 per cent., which loss was considered as water; the powdered mineral had become much darker in colour during ignition, and on cooling had assumed a reddish-brown tint.

In order to determine the amount of carbonic acid present, this gas was displaced by the action of hydrochloric acid upon 100 grs. of the powdered mineral placed in a previously tared thin glass apparatus, from which the gases were evolved after passing through a tube containing chloride of calcium. The loss of carbonic acid was found to be 0·45 gr., or equivalent to 0·98 per cent. carbonate of lime in the mineral.

Sulphuric acid was also distinctly observed, but in quantity too small for estimation unless an unusually large amount of the mineral had been operated upon.

For determining the fluorine a thin glass bulb was employed, closed by a cork through which two tubes passed, the one for evolving the gaseous products, and the other, reaching to the bottom of the bulb, for passing a stream of air through it in order to displace and drive off the hydrofluosilicic acid which remained behind in the apparatus at the termination of the operation; a sufficient quantity of pure strong sulphuric acid was now placed in the bulb along with 10 grains pure silicic acid (precipitated from hydrofluosilicic acid), and the whole boiled to expel any moisture present, and weighed. 50·00 grs. phosphorite in powder were then introduced, and the whole heated and weighed several times during two days; the ultimate loss was found to be 2·66, or 5·32 per cent. terfluoride of silicon; and as, according to Wöhler, 100 terfluoride of silicon is equivalent to 72·79 fluorine, this would afford 3·87 per cent. fluorine, or 8·01 fluoride of calcium.

The insoluble matter was estimated by dissolving 36·94 grs. in nitrohydrochloric acid and collecting the insoluble remainder, which was found to weigh 0·42 gr., or 1·41 per cent.

20·00 grs. were now digested with strong sulphuric acid, which evolved fumes of hydrofluoric acid, and the mass then

extracted with alcohol to dissolve the phosphoric and excess of sulphuric acid, and afterwards well washed with a saturated aqueous solution of sulphate of lime, after which the sulphate of lime was dried, incinerated, and weighed 23·03 grs.; deducting from this weight 0·15 for the insoluble matter contained in it, gives 22·88 grs. sulphate of lime, equivalent to 9·44 lime, or 47·29 per cent. lime. From the wash-water 0·07 gr. pyrophosphate of magnesia was obtained by the usual method, representing 0·025 gr. magnesia, or 0·12 per cent.

20·01 grs. were dissolved in NO^5, filtered from the insoluble residue, and the filtrate precipitated by a solution of nitrate of silver, which produced a minute precipitate, which on ignition gave 0·08 chloride of silver, equivalent to 0·02 chlorine, or 0·10 per cent. corresponding to 0·16 chloride of lime in the mineral. The excess of silver was removed as chloride from the filtrate, which now only gave, with chloride of barium, indications of sulphuric acid too small for accurate determination. The solution was now boiled and neutralized by carbonate of baryta, a solution of soda added in excess, and then a solution of carbonate of soda to precipitate all excess of baryta. The precipitate, which was now filtered off, evidently from its colour contained all the iron present, which was demonstrated when, on boiling the filtrate (previously acidified with a little hydrochloric acid) with a little chlorate of potash and precipitating with ammonia, no iron was obtained, and only pure alumina fell, which was collected, and, on ignition, gave 0·35 gr., or 1·75 per cent. in the phosphorite.

In order to determine the phosphoric acid, 14·28 grs. were dissolved in nitric acid filtered from insoluble matter, and a solution of molybdate of ammonia, previously supersaturated with nitric acid, added in great excess, and allowed to stand two days, taking the precaution to test whether sufficient molybdate of ammonia had been added. The beautiful yellow precipitate of phosphomolybdate of ammonia, after separation, was then dissolved off the filter by addition of ammonia, and this solution precipitated by chloride of magnesium. The precipitated phosphate of ammonia and magnesia was now collected and incinerated with the usual precautions, and gave 9·74 pyrophosphate of magnesia = 6·303 phosphoric acid, or 44·12 per cent.; the filtrate, which, after having separated the phosphoric acid by the molybdate of ammonia, still contained the bases, was placed in a corked flask with ammonia in excess and sulphide of ammonium also in excess, and allowed to stand some twelve hours. The insoluble matter containing the iron and alumina was then filtered off, washed with water containing sulphide of ammonium, and then dissolved in nitrohydrochloric acid; a little

pure sulphur was removed from this solution, and then ammonia added to precipitate the alumina and sesquioxide of iron. These were not separated, but after washing were ignited, and afforded a weight of 0·42 gr., equivalent to 2·94 per cent.; and subtracting from this the 1·75 per cent. alumina previously determined, we have 1·19 per cent. as the amount of sesquioxide of iron present in the mineral.

The results above, when tabulated, will give the following percentage composition:—

Fluoride of calcium	8·01
Chloride of calcium	0·16
Lime	41·03
Magnesia	0·12
Alumina	1·75
Sesquioxide of iron	1·19
Phosphoric acid	44·12
Sulphuric acid	trace
Carbonic acid	0·40
Insoluble matter	1·41
Water	1·44
	99·63

XLIX. *On the Theory of the Evolute.*

By A. Cayley, *F.R.S.**

ACCORDING to the generalized notion of geometrical magnitude, two lines are said to be at right angles to each other when they are harmonics in regard to a certain conic called the Absolute; this being so, the normal at any point of a curve is the line at right angles to the tangent, and the Evolute is the envelope of the normals.

Let the equation of the absolute be

$$\Theta = (a, b, c, f, g, h\,\text{\textasteriskcentered}\,x, y, z)^2 = 0,$$

and suppose, as usual, that the inverse coefficients are (A, B, C, F, G, H). Consider a given curve $U = (*\,x, y, z)^m = 0$, and suppose, for shortness, that the first differential coefficients of U are denoted by L, M, N. Then we have to find the equation of the normal at the point (x, y, z) of the curve $U = 0$.

The condition that any two lines are harmonics in regard to the absolute, is equivalent to this, viz. each line passes through the pole of the other line in regard to the absolute. Hence the normal at the point (x, y, z) is the line joining this point with the pole of the tangent. Now, taking (X, Y, Z) as current co-

* Communicated by the Author.

ordinates, the equation of the tangent is

$$LX+MY+NZ=0.$$

The coordinates of the pole of the tangent are therefore

$$(A, H, G\mathbin{)\!(}L, M, N) : (H, B, F\mathbin{)\!(}L, M, N) : (G, F, C\mathbin{)\!(}L, M, N).$$

And the equation of the normal is

$$\begin{vmatrix} X & , & Y & , & Z \\ x & , & y & , & z \\ (A, H, G\mathbin{)\!(}L, M, N), & (H, B, F\mathbin{)\!(}L, M, N), & (G, F, C\mathbin{)\!(}L, M, N) \end{vmatrix} =0.$$

The formula in this form will be convenient in the sequel; but there is no real loss of generality in taking the equation of the absolute to be $x^2+y^2+z^2=0$; the values of (A, B, C, F, G, H) are then (1, 1, 1, 0, 0, 0), and the formula becomes

$$\begin{vmatrix} X, & Y, & Z \\ x, & y, & z \\ L, & M, & N \end{vmatrix} =0;$$

where it will be remembered that (L, M, N) denote the derived functions $(\partial_x U, \partial_y U, \partial_z U)$.

The evolute is therefore the envelope of the line represented by the foregoing equation, say the equation $\Omega=0$, considering therein (x, y, z) as variable parameters connected by the equation $U=0$.

As an example, let it be required to find the evolute of a conic; since the axes are arbitrary, we may without loss of generality assume that the equation of the conic is $xz-y^2=0$. The values of (L, M, N) here are $(z, -2y, x)$. Moreover the equation is satisfied by writing therein $x : y : z = 1 : \theta : \theta^2$; the values of (L, M, N) then become $(\theta^2, -2\theta, 1)$ and the equation is

$$\begin{vmatrix} 1 & , & \theta & , & \theta^2 \\ X & , & Y & , & Z \\ (A, H, G\mathbin{)\!(}\theta, -1)^2, & (H, B, F\mathbin{)\!(}\theta, -1)^2, & (G, F, C\mathbin{)\!(}\theta, -1)^2 \end{vmatrix} =0;$$

or developing, this is

$$\begin{aligned} &X\left(\begin{matrix} G\theta^3-2F\theta^2+C\theta \\ -H\theta^4+2B\theta^3-F\theta^2 \end{matrix}\right) \\ &+Y\left(\begin{matrix} A\theta^4-2H\theta^3+G\theta^2 \\ -G\theta^2+2F\theta-C \end{matrix}\right) \\ &+Z\left(\begin{matrix} H\theta^2-2B\theta+F \\ -A\theta^3+2H\theta^2-G\theta \end{matrix}\right)=0, \end{aligned}$$

which I leave in this form in order to show the origin of the

different terms, and in particular in order to exhibit the destruction of the term θ^2 in the coefficient of Y. But the equation is, it will be observed, a quartic equation in θ, with coefficients which are linear functions of the current coordinates (X, Y, Z).

The equation shows at once that the evolute is of the class 4; in fact treating the coordinates (X, Y, Z) as given quantities, we have for the determination of θ an equation of the order 4, that is, the number of normals through a given point (X, Y, Z), or, what is the same thing, the class of the evolute, is $=4$.

The equation of the evolute is obtained by equating to zero the discriminant of the foregoing quartic function of θ; the order of the evolute is thus $=6$. There are no inflexions, and the diminution of the order from 4.3, $=12$, to 6 is caused by three double tangents.

I consider the particular case where the conic touches the absolute. There is no loss of generality in assuming that the contact takes place at the point $(y=0, z=0)$, the common tangent being therefore $z=0$; the conditions for this are $a=0$, $h=0$, and we have thence $C=0$, $F=0$. Substituting these values, the equation contains the factor θ; and throwing this out, it is

$$\begin{aligned} &X(-H\theta^3+(B+2G)\theta^2 \qquad\qquad\quad) \\ &+Y(\ A\theta^3- \quad 2H\theta^2 \qquad\quad) \\ &+Z(\qquad\quad -A\theta^2+3H\theta-(B+2G))=0, \end{aligned}$$

or, what is the same thing,

$$\begin{aligned} &\theta^3(-H \quad X+A\ Y \qquad\quad) \\ &+\theta^2((B+2G)X-2HY-A \quad Z) \\ &+\theta\ (\qquad\quad 3H \quad Z) \\ &+\ (\qquad\quad -(B+2G)Z)=0, \end{aligned}$$

where it will be observed that the constant term and the coefficient of θ have the same variable factor Z, where $Z=0$ is the equation of the common tangent of the conic and the absolute. The evolute is in this case of the class 3. It at once appears that the line $Z=0$ is a stationary tangent of the evolute, the point of contact or inflexion on the evolute being given by the equations $Z=0$, $(B+2G)X-2HY=0$. The equation of the evolute is found by equating to zero the discriminant of the cubic function; the equation so obtained has the factor Z, and throwing this out the order is $=3$. The evolute is thus a curve

of the class 3 and order 3, the reduction in the order from 3.2, =6, to 3 being caused by the existence of an inflexion. Comparing with the former case, we see that the effect of the contact of the conic with the absolute is to give rise to an inflexion of the evolute, and to cause a reduction =1 in the class, and a reduction =3 in the order.

I return now to the general case of a curve

$$U = (*)(x, y, z)^m = 0.$$

Using, for greater simplicity, the equation $x^2+y^2+z^2=0$ for the absolute, the equation of the normal is

$$\Omega = \begin{vmatrix} X & , Y & , Z \\ x & , y & , z \\ \partial_x U, & \partial_y U, & \partial_z U \end{vmatrix} = 0;$$

we may at once find the class of the evolute; in fact, treating (X, Y, Z) as the coordinates of a given point, the two equations $U=0$, $\Omega=0$ determine the values (x, y, z) of the coordinates of a point such that the normal thereof passes through the point (X, Y, Z); the number of such points is the number of normals which can be drawn through a given point (X, Y, Z), viz. it is equal to the class of the evolute. The points in question are given as the intersections of the two curves $U=0$, $\Omega=0$, which are respectively curves of the order m, hence the number of intersections is $=m^2$. It is to be observed, however, that if the curve $U=0$ has nodes or cusps, then the curve $\Omega=0$ passes through each node of the curve $U=0$, and through each cusp, the two curves having at the cusp a common tangent; that is, each node reckons for two intersections, and each cusp for three intersections. Hence, if the curve $U=0$ has δ nodes and κ cusps, the number of the remaining points of intersection is $=m^2-2\delta-3\kappa$. The class of the evolute is thus $=m^2-2\delta-3\kappa$. The number of inflexions is in general $=0$. If, however, the given curve touches the absolute, then it has been seen in a particular case that the effect is to diminish the class by 1, and to give rise to an inflexion, the stationary tangent being in fact the common tangent of the curve and the absolute: I assume that this is the case generally. Suppose that there are θ contacts, then there will be a diminution $=\theta$ in the class, or this will be $=m^2-2\delta-3\kappa-\theta$; and there will be θ inflexions; there may however be special circumstances giving rise to fresh inflexions, and I will therefore assume that the number of inflexions is $=\iota'$.

Suppose in general that for any curve we have

m, the order,
n, „ class,
δ, „ number of nodes,
κ, „ „ cusps,
τ, „ „ double tangents,
ι, „ „ inflexions.

Then Plücker's equations give

$$\iota-\kappa=3(n-m),\quad \tau-\delta=\tfrac{1}{2}(n-m)(n+m-9);$$

and we thence have

$$\iota-\kappa+\tau-\delta=\tfrac{1}{2}(n-1)(n-2)-\tfrac{1}{2}(m-1)(m-2),$$

or, what is the same thing,

$$\tfrac{1}{2}(m-1)(m-2)-\delta-\kappa=\tfrac{1}{2}(n-1)(n-2)-\tau-\iota.$$

Now M. Clebsch in his recent paper "Ueber die Singularitäten algebraischer Curven," *Crelle*, vol. lxiv. (1864) pp. 98–100, has remarked (as a consequence of the investigations of Riemann in the Integral Calculus) that whenever from a given curve another curve is derived in such manner that to each point (or tangent) of the given curve there corresponds *a single* tangent (or point) of the derived curve, then the expression

$$\tfrac{1}{2}(m-1)(m-2)-\delta-\kappa,\quad =\tfrac{1}{2}(n-1)(n-2)-\tau-\iota,$$

has the same value in the two curves respectively, or that, writing m', n', δ', κ', τ', ι' for the corresponding quantities in the second curve, then we have

$$\tfrac{1}{2}(m-1)\,(m-2)-\delta-\kappa=\tfrac{1}{2}(n-1)\,(n-2)-\tau-\iota$$
$$=\tfrac{1}{2}(m'-1)(m'-2)-\delta'-\kappa'=\tfrac{1}{2}(n'-1)(n'-2)-\tau'-\iota';$$

and consequently that, knowing any *two* of the quantities m', n', δ', κ', τ', ι', the remainder of them can be determined by means of this relation and of Plücker's equations. The theorem is applicable to the evolute according to the foregoing generalized definition*; and starting from the values

$$n'=m^2-2\delta-3\kappa-\theta,$$
$$\iota'=\iota,$$

we find in the first instance

$$\tau'=\tfrac{1}{2}(n'-1)(n'-2)-\tfrac{1}{2}(m-1)(m-2)+\delta+\kappa-\iota';$$

* M. Clebsch in fact applies it to the evolute in the ordinary sense of the term, but by inadvertently assuming $\iota'=k$ instead of $\iota=0$ he is led to some incorrect results.

and substituting in the equation

$$m' = n'(n'-1) - 2\tau' - 3\iota',$$

we find

$$m' = 2(n'-1) + (m-1)(m-2) - 2\delta - 2\kappa - \iota';$$

and the equation $\iota' - \kappa' = 3(n'-m')$ gives also

$$\kappa' = -3(n'-m') + \iota',$$

whence, attending to the value of n', we find the following system of equations for the singularities of the evolute, viz.

$$\begin{aligned} n' &= m^2-2 && \delta-3\ \kappa-\ \theta, \\ m' &= 3m(m-1)-6 && \delta-8\ \kappa-2\theta-\iota', \\ \iota' &= \quad \iota', \\ \kappa' &= 3m(2m-3)-12\delta-15\kappa-3\theta-2\iota', \end{aligned}$$

and the values of τ' and δ' may then also be found from the equations

$$\begin{aligned} m' &= n'(n'-1)-2\tau'-3\iota', \\ n' &= m'(m'-1)-2\delta'-3\kappa'. \end{aligned}$$

I have given the system in the foregoing form, as better exhibiting the effect of the inflexions; but as each of the θ contacts with the absolute gives an inflexion, we may write $\iota' = \theta + \iota''$, where, in the absence of special circumstances giving rise to any more inflexions, $\iota'' = 0$. The system thus becomes

$$\begin{aligned} n' &= m^2-2 && \delta-3\ \kappa-\ \theta, \\ m' &= 3m(m-1)-6 && \delta-8\ \kappa-3\theta-\iota'', \\ \iota' &= && \theta+\iota'', \\ \kappa' &= 3m(2m-3)-12\delta-15\kappa-5\theta-2\iota'', \end{aligned}$$

so that each contact with the absolute diminishes the class by 1, the order by 3, and the number of cusps by 5.

I remark that when the absolute becomes a pair of points, a contact of the given curve m means one of two things: either the curve touches the line through the two points, or else it passes through one of the two points: the effect of a contact of either kind is as above stated. Suppose that the two points are the circular points at infinity, and let $m=2$, the evolute in question is then the evolute of a conic, in the ordinary sense of the word evolute. We have, in general, class $=4$, order $=6$; but if the conic touches the line infinity (that is, in the case of the parabola), the reductions are 1 and 3, and we have class $=3$, order $=3$, which is right. If the conic passes through one of the circular points of infinity, then in like manner the reductions are 1 and 3; and therefore if the conic passes through each of the circular points at infinity (that is, in the case of a circle),

the reductions are 2 and 6, and we have class $=2$, order $=0$, which is also right; for the evolute is in this case the centre, regarded as a pair of coincident points. That this is so, or that the class is to be taken to be (not $=1$ but) $=2$, appears by the consideration that the number of normals to the circle from a given point is in fact $=2$, the two normals being, however, coincident in position.

To complete the theory in the general case where the absolute is a proper conic, I remark that, besides the inflexions which arise from contacts of the given curve with the absolute, there will be an inflexion, first, for each stationary tangent of the given curve which is also a tangent of the absolute; secondly, for each cusp of the given curve situate on the absolute. Hence, if the number of such stationary tangents be $=\lambda$, and the number of such cusps be $=\mu$, we may write $\iota''=\lambda+\mu$, and therefore also $\iota'=\theta+\lambda+\mu$.

I remark also that we have

$$-2\delta-3\kappa=-\ m(m-1)+n,$$
$$-6\delta-8\kappa=-3m(m-2)+i,$$

and therefore also

$$-12\delta-15\kappa=-6m^2+15m-3n+3i.$$

The general formulæ thus become

$$\begin{aligned} n' &= m+\ n\ \ -\theta\ \ , \\ m' &= 3m\ \ +\ \iota-2\theta-\ \iota', \\ \iota' &= \qquad\qquad\ \ \iota', \\ \kappa' &= 6m-3n+3\iota-3\theta-2\iota'. \end{aligned}$$

If instead of the given curve we consider its reciprocal in regard to the absolute, then

$$m,\ n,\ \delta,\ \kappa,\ \tau,\ \iota;\quad \theta,\ \lambda,\ \mu;\quad \iota'=\theta+\lambda+\mu$$

are changed into

$$n,\ m,\ \tau,\ \iota,\ \delta,\ \kappa;\quad \theta,\ \mu,\ \lambda;\quad \iota'=\theta+\mu+\lambda$$

respectively. And for the evolute of the reciprocal curve we have

$$\begin{aligned} n' &= n+\ m\ \ -\theta\ \ \iota', \\ m' &= 3n\ \ +\ \kappa-2\theta-\iota', \\ \iota' &= \qquad\qquad\ \ \iota', \\ \kappa' &= 6n-3m+3\kappa-3\theta-\iota', \end{aligned}$$

which, attending to the relation $\iota-\kappa=3(n-m)$, are in fact the same as the former values; that is, the evolute of the given curve, and the evolute of the reciprocal curve are curves of the same class and order, and which have the same singularities.

Cambridge, February 22, 1865.

L. *On the Application of Screw-Blades as Floats for Paddle-wheels.* *By* W. G. Adams, *M.A., F.G.S., Fellow of St. John's College, Cambridge, Professor of Natural Philosophy in King's College, London**.

SINCE my former paper on this subject was communicated to the Philosophical Magazine, experiments (*an account of which appeared in the 'Times' of the 5th of April*) have been made at Portsmouth with Dr. Croft's new wheel, on Her Majesty's private yacht 'Elfin,' with very satisfactory results, showing in the very first trial that the new wheel is superior to the wheel with ordinary flat floats, and very nearly comes up to the wheel with feathering floats, even with a much smaller surface exposed to the pressure of the water.

One of the points especially worthy of notice is the fact that "*no vibration whatever was experienced on board*"; so that it is no longer a mere matter of theory, but also true in practice, that with the new wheel the sudden jerking and quivering motion have no existence. In my former paper it is stated that if the number of revolutions be such that v is greater than $\frac{\omega r}{2}$, then the part of the float nearest to the axis will tend to stop rather than to propel the boat. This must certainly be the case where the radius of the paddle-wheel is small, as in the case of the 'Elfin,' and may also be the case in a larger vessel. This difficulty may be obviated by cutting away the part of the float nearest to the axis; and I propose to consider the effect of such a change on the resistance and moment about the axis, in order to determine whether the float is rendered thereby more or less efficient. I also propose to investigate the expressions for the resistance and moment on a screw-blade whose pitch is different from 45°, and to determine the efficiency when the pitch is what is practically considered most advantageous. The equations giving the resistance and moment on one float are these:—

$$\mathrm{R} = \tfrac{1}{2}\rho_1\omega^2 r^3 \iint \frac{(\rho + a\cos\theta)^2\cos\theta}{r^2+\rho^2}\,d\rho\,d\theta$$

$$-\tfrac{1}{2}\rho_1 v\omega r^3 \iint \frac{2\rho\cos^2\theta}{r^2+\rho^2}\,d\rho\,d\theta$$

$$-\tfrac{1}{2}\rho_1 v(2\omega a - v) r^3 \iint \frac{\cos^3\theta}{r^2+\rho^2}\,d\rho\,d\theta$$

* Communicated by the Author.

$$= \tfrac{1}{2}\rho_l\omega^2 r^3 \Big\{ \int \rho \cos\theta\, d\theta + \int a\cos^2\theta \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta$$
$$- \int \frac{r^2 - a^2\cos^2\theta}{r} \cos\theta \tan^{-1}\frac{\rho}{r}\, d\theta \Big\}$$
$$- \tfrac{1}{2}\rho_l v\omega r^3 \int \cos^2\theta \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta - \tfrac{1}{2}\rho_l v(2\omega a - v) r^3 \int \frac{\cos^3\theta}{r} \tan^{-1}\frac{\rho}{r}\, d\theta$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^4 \Big\{ \int \frac{\rho}{r}\cos\theta\, d\theta + \int \frac{a}{r}\cos^2\theta \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta$$
$$- \int \Big(1 - \frac{a^2}{r^2}\cos^2\theta\Big) \cos\theta \tan^{-1}\frac{\rho}{r}\, d\theta$$
$$- \frac{v}{\omega r} \int \cos^2\theta \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta - \frac{v}{\omega r}\Big(\frac{2a}{r} - \frac{v}{\omega r}\Big) \int \cos^3\theta \tan^{-1}\frac{\rho}{r}\, d\theta \Big\}.$$

$$\mathrm{M} = \tfrac{1}{2}\rho_l\omega^2 r^3 \iint \frac{(\rho + a\cos\theta)^3}{r^2+\rho^2}\, d\rho\, d\theta$$
$$- \rho_l v\omega r^3 \iint \frac{(\rho + a\cos\theta)^2 \cos\theta}{r^2+\rho^2}\, d\rho\, d\theta$$
$$+ \tfrac{1}{4}\rho_l v^2 r^3 \iint \frac{2\rho\cos^2\theta}{r^2+\rho^2}\, d\rho\, d\theta + \tfrac{1}{2}\rho_l v^2 a r^3 \iint \frac{\cos^3\theta}{r^2+\rho^2}\, d\rho\, d\theta$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^3 \Big\{ \int \tfrac{1}{2}\rho^2 d\theta + \int 3a\rho\cos\theta\, d\theta - \int \frac{r^2 - 3a^2\cos^2\theta}{2} \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta$$
$$- \int \frac{3ar^2\cos\theta - a^3\cos^3\theta}{r} \tan^{-1}\frac{\rho}{r}\,.\,d\theta \Big\}$$
$$- \tfrac{1}{2}\rho_l 2v\omega r^3 \Big\{ \int \rho\cos\theta\, d\theta + \int a\cos^2\theta \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta$$
$$- \int \frac{r^2 - a^2\cos^2\theta}{r} \cos\theta \tan^{-1}\frac{\rho}{r}\,.\,d\theta \Big\}$$
$$+ \tfrac{1}{4}\rho_l v^2 r^3 \int \cos^2\theta \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta + \tfrac{1}{2}\rho_l v^2 a r^2 \int \cos^3\theta \tan^{-1}\frac{\rho}{r}\,.\,d\theta$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^5 \Big\{ \int \frac{1}{2}\cdot\frac{\rho^2}{r^2}\, d\theta + \int 3\cdot\frac{a}{r}\cdot\frac{\rho}{r}\cos\theta\, d\theta - \int \frac{1 - \dfrac{3a^2}{r^2}\cos^2\theta}{2} \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta$$
$$- \int \Big(3 - \frac{a^2}{r^2}\cos^2\theta\Big) \frac{a}{r}\cos\theta\,.\tan^{-1}\frac{\rho}{r}\,.\,d\theta$$
$$- \frac{2v}{\omega r}\Big[\int \frac{\rho}{r}\cos\theta\, d\theta + \int \frac{a}{r}\cos^2\theta \log\Big(1+\frac{\rho^2}{r^2}\Big) d\theta$$
$$- \int \Big(1 - \frac{a^2}{r^2}\cos^2\theta\Big) \cos\theta \tan^{-1}\frac{\rho}{r}\, d\theta \Big]$$
$$+ \frac{1}{2}\cdot\frac{v^2}{\omega^2 r^2} \int \cos^2\theta \log\Big(1 - \frac{\rho^2}{r^2}\Big) d\theta + \frac{v^2}{\omega^2 r^2}\cdot\frac{a}{r} \int \cos^3\theta \tan^{-1}\frac{\rho}{r}\, d\theta \Big\}.$$

Suppose that a portion of the screw-blade nearest the axis of breadth kr is cut away, then this will be the lower limit for ρ in the integration.

If only that part of the float were cut away which tends to stop the boat, then the lower limit of integration would be given by the equation $\omega(\rho_0 + a\cos\theta) - v\cos\theta = 0$, or

$$\rho_0 = \left(\frac{v}{\omega} - a\right)\cos\theta$$

$$= \left(\frac{v}{\omega r} - \frac{a}{r}\right) r\cos\theta.$$

Or if the part cut away be of the same width throughout, then

$$\rho_0 = \left(\frac{v}{\omega r} - \frac{a}{r}\right) r,$$

or

$$\left(\frac{v}{\omega r} - \frac{a}{r}\right) = k.$$

Supposing, as before, that $a = \frac{r}{2}$, then

$$\frac{v}{\omega r} = \tfrac{1}{2}(1 + 2k).$$

Without these suppositions the quantities k, $\frac{a}{r}$, and $\frac{v}{\omega r}$ are independent of one another; and it may be considered doubtful whether (1) they should have the same values for wheels of different radii, or (2) whether the above are the best hypotheses which can be made. It is only by repeating the calculations for different cases that these points can be theoretically determined.

Taking the above values of $\frac{a}{r}$ and $\frac{v}{\omega r}$, the resistance on both floats becomes

$$\tfrac{1}{2}\rho_1\omega^2 r^4 \left\{ 2k\int\cos\theta\, d\theta + \log_e(1+k^2)\int\cos^2\theta\, d\theta \right.$$

$$-2\tan^{-1}k\int\cos\theta\, d\theta + \tfrac{1}{2}\tan^{-1}k\int\cos^3\theta\, d\theta$$

$$-(1+2k)\log_e(1+k^2)\int\cos^2\theta\, d\theta$$

$$\left. -\tfrac{1}{2}\tan^{-1}k\int\cos^3\theta\, d\theta + 2k^2.\tan^{-1}k\int\cos^3\theta\, d\theta \right\}$$

$$= \tfrac{1}{2}\rho_l\omega^2 r^4 \{2k\sin\theta - k\log_e(1+k^2)(\theta+\tfrac{1}{2}\sin 2\theta) - 2\tan^{-1}k\sin\theta$$
$$+ 2k^2\tan^{-1}k\,(\sin\theta - \tfrac{1}{3}\sin^3\theta)\}$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^4 \{(k - \tan^{-1}k)\times\cdot68404 - k\log_e(1+k^2)\times\cdot67046$$
$$+ k^2\tan^{-1}k\times\cdot65737\}$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^4 \{k^3(\cdot22801 - \cdot67046 + \cdot65737)$$
$$- k^5(\cdot13681 - \cdot33523 + \cdot21912) + \&c.\}$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^4 \{k^3\times\cdot21492 - k^5\times\cdot0207\}.$$

The moment on the two floats becomes

$$\tfrac{1}{2}\rho_l\omega^2 r^5 \{k^2\theta + 3(k-\tan^{-1}k)\sin\theta - \log_e(1+k^2)\times\theta$$
$$+ \tfrac{3}{8}\log_e(1+k^2)(\theta+\tfrac{1}{2}\sin 2\theta) + \tfrac{1}{4}\tan^{-1}k\,(\sin\theta - \tfrac{1}{3}\sin^3\theta)$$
$$- (1+2k)[(k-\tan^{-1}k)2\sin\theta + \tfrac{1}{2}\log_e(1+k^2)(\theta+\tfrac{1}{2}\sin 2\theta)$$
$$+ \tfrac{1}{2}\tan^{-1}k\,(\sin\theta - \tfrac{1}{3}\sin^3\theta)]$$
$$+ \tfrac{1}{8}(1+2k)^2[\log_e(1+k^2)(\theta+\tfrac{1}{2}\sin 2\theta) + 2\tan^{-1}k(\sin\theta - \tfrac{1}{3}\sin^3\theta)]\}$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^5 \{(k-\tan^{-1}k)(\sin\theta - 4k\sin\theta) + \left(k^2 - \log_e(1+k^2)\right)\times\theta$$
$$- \tfrac{1}{2}k\log_e(1+k^2)(\theta+\tfrac{1}{2}\sin 2\theta) + \tfrac{1}{2}k^2\log_e(1+k^2)(\theta+\tfrac{1}{2}\sin 2\theta)$$
$$+ k^2\tan^{-1}k\,(\sin\theta - \tfrac{1}{3}\sin^3\theta)\}$$

$$= \tfrac{1}{2}\rho_l\omega^2 r^5 \left\{\left(\frac{k^3}{3} - \frac{k^5}{5}\right)(1-4k)\sin\theta + \frac{k^4}{2}\theta - \left(\frac{k^3}{2} - \frac{k^5}{4}\right)(\theta+\tfrac{1}{2}\sin 2\theta)\right\}$$
$$+ \frac{k^4}{2}(\theta+\tfrac{1}{2}\sin 2\theta) + \left(k^3 - \frac{k^5}{3}\right)(\sin\theta - \tfrac{1}{3}\sin^3\theta)\Big\}$$

$$= \tfrac{1}{2}\rho_l\omega^2 r^5 \{k^3(\cdot11401 - \cdot33523 + \cdot32868)$$
$$+ k^4(-\cdot45604 + \cdot17453 + \cdot33523)$$
$$- k^5(\cdot06840 - \cdot16761 + \cdot10956)\}$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^5 \{k^3\times\cdot10746 + k^4\times\cdot05372 - k^5\times\cdot01035\}.$$

Hence the resistance on a pair of floats

$$= \tfrac{1}{2}\rho_l\omega^2 r^4 \{\cdot513256 - (1+2k)\times\cdot234563 - \tfrac{1}{2}(1-4k^2)\times\cdot259202$$
$$- k^3\times\cdot21492 + k^5\times\cdot0207\}$$
$$= \tfrac{1}{2}\rho_l\omega^2 r^4 \{\cdot149092 - k\times\cdot469126 + k^2\times\cdot518404 - k^3\times\cdot21492$$
$$+ k^5\times\cdot0207\}.$$

And the moment of the resistance on them

$$= \tfrac{1}{2}\rho_l\omega^2 r^5\{\cdot 573869 - (1+2k)\times\cdot 513256 + \tfrac{1}{4}(1+2k)^2\times\cdot 493765 - k^3\times\cdot 10746 - k^4\times\cdot 05372 + k^5\times\cdot 01035\}$$

$$= \tfrac{1}{2}\rho_l\omega^2 r^5\{\cdot 184054 - k\times\cdot 532747 + k^2\times\cdot 493765 - k^3\times\cdot 10746 - k^4\times\cdot 05372 + k^5\times\cdot 01035\}.$$

1. When the vessel is going at a uniform speed, the amount of useful work done by the resistance in a unit of time is Rv, and the amount done by the engines is $M\omega$, therefore $\frac{Rv}{M\omega}$ is a measure of the efficiency of the paddle-wheel.

(1) In the case investigated in my former paper $k=0$; therefore

$$\frac{Rv}{M\omega} = \frac{\cdot 149092}{\cdot 184054}\times\frac{1}{2} = \cdot 405.$$

(2) Now, if we suppose that all the portions of the floats which are within a distance of half the radius from the centre of the wheel to be cut away, then $k=\frac{1}{3}$. Substituting this value for k we get

$$\text{Resistance} = \tfrac{1}{2}\rho_l\omega^2 r^4\times\cdot 04266,$$

$$\text{Moment} = \tfrac{1}{2}\rho_l\omega^2 r^5\times\cdot 05674.$$

Now

$$\frac{v}{\omega} = \frac{r}{2}(1+2k).$$

And when the vessel is going at full speed, the measure of the efficiency is

$$\frac{Rv}{M\omega} = \frac{1}{2}\times\frac{\cdot 04266}{\cdot 05674}\times(1+2k) = \frac{5}{6}\times\frac{4266}{5674} = \cdot 6265.$$

(3) If we suppose that the innermost portion of the float is at a distance from the centre equal to two-thirds of the radius of the wheel, then $k=\frac{1}{2}$, and we get

$$\text{Resistance} = \tfrac{1}{2}\rho_l\omega^2 r^4\times\cdot 01856,$$

$$\text{Moment} = \tfrac{1}{2}\rho_l\omega^2 r^5\times\cdot 02465.$$

Also

$$\frac{v}{\omega} = \frac{r}{2}(1+2k) = r;$$

therefore the efficiency $= \frac{1856}{2465} = \cdot 753$.

If the velocity of the vessel be 10 knots an hour or 1000 feet

in a minute nearly, and n the number of revolutions a minute, then $2\pi n=\omega$ and

$$\frac{r}{2}(1+2k)=\frac{1000}{2\pi n}.$$

If, as in the case of the 'Elfin,' the radius of the wheel is $5\frac{1}{2}$ feet nearly, then

$$r=\tfrac{2}{3}\text{ of }5\tfrac{1}{2}=\tfrac{11}{3},$$

therefore

$$1+2k=\frac{3000}{11\pi n}=\frac{90}{n}\text{ nearly};$$

therefore if

$$n=45,\quad k=\tfrac{1}{2},$$

showing that, if the number of revolutions a minute be 45, then all that portion of the blade should be cut away which is within a distance of the axis of the *wheel* equal to two-thirds of the radius; *i. e.*, half the blade should be cut away.

In the trials with the 'Elfin' (*see the report in the 'Times' of April 5th*) the area of immersed screw-blades when complete was 72 feet; hence *theoretically*, with forty-six revolutions a minute, rather more than half the blade should be cut away, leaving an area of about 35 feet; *practically* it was found that the speed was the greatest when the area was reduced to 32·7 feet, showing a very close agreement between the theoretical and experimental results.

(4) If the radius of the paddle-wheel be 9 feet, then $r=6$ feet, and

$$\frac{v}{\omega}=3(1+2k).$$

If we again suppose the velocity to be 10 knots an hour or 1000 feet in a minute, and the number of revolutions a minute to be 33, then

$$\frac{v}{\omega}=\frac{1000}{200}=5,$$

and $k=\frac{1}{3}$, or one-third of the float should be cut away. If the number of revolutions a minute be only 28, then

$$\frac{v}{\omega}=\frac{1000}{166}=6\text{ nearly},$$

and $k=\frac{1}{2}$, showing that in this case one half of the blade should be removed, or that no part of it should be at a less distance from the centre than two-thirds of the radius of the wheel.

If $\frac{v}{\omega}=4\frac{1}{2}$, then $k=\frac{1}{4}$, and for a velocity of 10 knots an hour

the number of revolutions a minute must be 36. The efficiency

$$\frac{Rv}{M\omega} = \frac{\cdot 060871}{\cdot 079837} \times \frac{v}{\omega r} = \frac{\cdot 060871}{\cdot 079837} \times \frac{3}{4} = \cdot 572.$$

(5) In the case of the flat float with paddle-wheels of the same radius and making the same number of revolutions with the same speed, the efficiency

$$\frac{Rv}{M\omega} = \frac{\cdot 0234}{\cdot 021} \times \frac{v}{\omega r} = \frac{\cdot 0234}{\cdot 021} \times \frac{1}{2} = \cdot 557;$$

so that the flat float is *not* so advantageous as the screw-blade with the portion nearest the axis cut away.

Cases (4) and (5) may be compared with the case which follows, so as to determine whether it is more advantageous to have screw-blades with the inner portions cut out, or complete screw-blades of the same size, but having their axis at a distance from the centre of the wheel greater than one-third of the radius.

Another point to be considered is, what should be the pitch of the screw-blade at the circumference of the wheel? If γ be the pitch, then the blade extends to a distance $(a + r\cot\gamma)$ from the axis of the *wheel*, $a + r$ being, as in the case already considered, the distance from the axis to the point where the pitch is 45°. Therefore for the superior limit of ρ in this case

$$a^2 + \rho^2 + 2a\rho\cos\theta = (a + r\cot\gamma)^2 = r^2\cot^2\gamma(1+\lambda)^2$$

[where $a = \lambda r\cot\gamma$],

or

$$\frac{\rho}{r} = (\sqrt{1 + 2\lambda + \lambda^2\cos^2\theta} - \lambda\cos\theta)\,.\,\cot\gamma.$$

Considering the float to be a complete screw surface having its axis at a distance $a = r\lambda\cot\gamma$ from the centre of the wheel, the lower limit for ρ will be 0.

Let

$$(\sqrt{1 + 2\lambda + \lambda^2\cos^2\theta} - \lambda\cos\theta)\cot\gamma = q.$$

Then the expressions for the resistance and moment on one float are these:

$$\begin{aligned} R = \tfrac{1}{2}\rho_1\omega^2 r^4 \Big\{ & \int q\cos\theta\,d\theta + \int \lambda\cot\gamma\cos^2\theta\log(1+q^2)\,d\theta \\ & - \int (1 - \lambda^2\cot^2\gamma\cos^2\theta)\cos\theta\,.\tan^{-1}q\,.\,d\theta \\ & - \frac{v}{\omega r}\int \cos^2\theta\log(1+q^2)\,d\theta \\ & - \frac{v}{\omega r}\left(2\lambda\cot\gamma - \frac{v}{\omega r}\right)\int \cos^3\theta\,.\tan^{-1}q\,.\,d\theta \Big\} \end{aligned}$$

$$= \tfrac{1}{2}\rho_l \omega^2 r^4 \Big\{ \int q \cos\theta d\theta - \int \tan^{-1} q \cos\theta d\theta$$
$$+ \left(\lambda \cot\gamma - \frac{v}{\omega r}\right) \int \cos^2\theta \log(1+q^2) d\theta$$
$$+ \left(\lambda \cot\gamma - \frac{v}{\omega r}\right)^2 \int \cos^3\theta \,.\, \tan^{-1} q d\theta \Big\},$$

$$\mathrm{M} = \tfrac{1}{2}\rho_l \omega^2 r^5 \Big\{ \int \frac{q^2}{2} d\theta + \int 3\lambda q \cot\gamma \cos\theta d\theta$$
$$- \int \tfrac{1}{2}(1 - 3\lambda^2 \cot^2\gamma \cos^2\theta) \log(1+q^2)\, d\theta$$
$$- \int (3 - \lambda^2 \cot^2\gamma \cos^2\theta) \,.\, \lambda \cot\gamma \cos\theta \tan^{-1} q d\theta$$
$$- \frac{2v}{\omega r}\Big[\int q \cos\theta d\theta + \int \lambda \cot\gamma \cos^2\theta \log(1+q^2) d\theta$$
$$- \int (1 - \lambda^2 \cot^2\gamma \cos^2\theta) \cos\theta \tan^{-1} q d\theta \Big]$$
$$+ \tfrac{1}{2} \,.\, \frac{v^2}{\omega^2 r^2} \int \cos^2\theta \log(1+q^2)\, d\theta$$
$$+ \frac{v^2}{\omega^2 r^2} \,.\, \lambda \cot\gamma \int \cos^3\theta \tan^{-1} q \,.\, d\theta \Big\}$$

$$= \tfrac{1}{2}\rho_l \omega^2 r^5 \Big\{ \int \frac{q^2}{2} d\theta - \int \tfrac{1}{2} \log(1+q^2) d\theta$$
$$+ \int \lambda \cot\gamma \,.\, q \cos\theta d\theta - \int \lambda \cot\gamma \,.\, \tan^{-1} q \,.\, \cos\theta d\theta$$
$$+ 2\left(\lambda \cot\gamma - \frac{v}{\omega r}\right)\Big[\int q \cos\theta d\theta - \int \tan^{-1} q \cos\theta \,.\, d\theta \Big]$$
$$+ \tfrac{1}{2}\left(\lambda \cot\gamma - \frac{v}{\omega r}\right)\left(3\lambda \cot\gamma - \frac{v}{\omega r}\right) \int \cos^2\theta \log(1+q^2)\, d\theta$$
$$+ \lambda \cot\gamma \left(\lambda \cot\gamma - \frac{v}{\omega r}\right)^2 \cdot \int \cos^3\theta \,.\, \tan^{-1} q d\theta \Big\}.$$

These equations for R and M will apply to all cases by giving different values to λ, γ, and the limit of θ, and by putting $q = k$ if any portion of the screw-blade of width kr be cut away,—remembering that the radius of the wheel $= r \cot\gamma(1+\lambda)$, and that the axis of the screw-blade is at a distance equal to $\lambda r \cot\gamma$

from the axis of the wheel. It may be noticed that if the velocity of the vessel be equal to the velocity of rotation of the edges of the blade nearest to the axis, then

$$\frac{v}{\omega r} = \lambda \cot \gamma.$$

In practice it is found that the efficiency of a screw is greatest when its pitch is about 25°; with this pitch the limit of θ must be increased, otherwise the breadth of the wheel will be diminished; also it appears from the above cases, and from the experiments already made, that the distance from the axis of the screw-surface to the axis of the wheel should not be less than half the radius of the wheel.

If then we take $\lambda = 1$ and $\gamma = 26\frac{1}{2}°$, so that $\cot \gamma = 2$, then

$$q = 2(\sqrt{3 + \cos^2 \theta} - \cos \theta).$$

If the limit for θ be 25°, then the breadth of the wheel will be $\frac{1}{1}\frac{5}{6}$ths of the breadth in the case which has been already investigated.

With these values of λ and γ we get

$$\mathrm{R} = \tfrac{1}{2}\rho_l \omega^2 r^4 \Big\{ \int q \cos\theta \, d\theta - \int \tan^{-1} q \,.\, \cos\theta \, d\theta + \left(2 - \frac{v}{\omega r}\right) \int \cos^2\theta \log(1+q^2)\, d\theta + \left(2 - \frac{v}{\omega r}\right)^2 \int \cos^3\theta \,.\, \tan^{-1} q \,.\, d\theta \Big\}.$$

$$\mathrm{M} = \tfrac{1}{2}\rho_l \omega^2 r^5 \Big\{ \int \frac{q^2}{2}\, d\theta + \int 2q \cos\theta \, d\theta - \int 2 \tan^{-1} q \,.\, \cos\theta \, d\theta - \int \tfrac{1}{2} \log(1+q^2)\, d\theta + 2\left(2 - \frac{v}{\omega r}\right)\Big[\int q \cos\theta \, d\theta - \int \tan^{-1} q \,.\, \cos\theta \, d\theta \Big] + \tfrac{1}{2}\left(2 - \frac{v}{\omega r}\right)\left(6 - \frac{v}{\omega r}\right) \int \cos^2\theta \log(1+q^2)\, d\theta + 2\left(2 - \frac{v}{\omega r}\right)^2 \int \cos^3\theta \,.\, \tan^{-1} q \, d\theta \Big\}.$$

Values of the Functions to be integrated, the limits of integration being 0° *and* 25°.

	$\cos\theta$.	q.	$q\cos\theta$.	$\tan^{-1}q\,.\cos\theta$.
°				
$2\frac{1}{2}$	·999048	2·000954	1·999050	1·106286
$7\frac{1}{2}$	·991445	2·008582	1·991398	1·099373
$12\frac{1}{2}$	·976296	2·025916	1·977894	1·085919
$17\frac{1}{2}$	·953717	2·047096	1·952350	1·064724
$22\frac{1}{2}$	·923879	2·078336	1·920132	1·036906
$27\frac{1}{2}$	·887011	2·117912	1·878611	1·002026

	q^2.	$\log_e(1+q^2)$.	$\log_e(1+q^2)\,.\cos^2\theta$.	$\tan^{-1}q\,.\cos\theta$.
°				
$2\frac{1}{2}$	4·003818	1·610201	1·607138	1·104182
$7\frac{1}{2}$	4·034400	1·616294	1·588757	1·080642
$12\frac{1}{2}$	4·104337	1·630090	1·553727	1·035043
$17\frac{1}{2}$	4·190604	1·646850	1·497935	·968447
$22\frac{1}{2}$	4·319480	1·671375	1·426608	·885054
$27\frac{1}{2}$	4·485553	1·702117	1·339206	·788382

Hence, by the same method as in my former paper,

$$\mathrm{R}=\tfrac{1}{2}\rho_l\omega^2r^4\left\{\cdot388103+\left(2-\frac{v}{\omega r}\right)\times\cdot669379+\left(2-\frac{v}{\omega r}\right)^2\times\cdot442383\right\}.$$

$$\mathrm{M}=\tfrac{1}{2}\rho_l\omega^2r^5\left\{1\cdot320900+\left(2-\frac{v}{\omega r}\right)\times\cdot776207+\left(2-\frac{v}{\omega r}\right)\left(6-\frac{v}{\omega r}\right)\times\cdot334690+\left(2-\frac{v}{\omega r}\right)^2\times\cdot884766\right\}.$$

(6) If the radius of the paddle-wheel be 9 feet, then

$$(1+\lambda)r\cot\gamma=9,\ \text{and}\ r=\tfrac{9}{4},$$

or r equals $\frac{1}{4}$th of the radius.

Supposing the speed of the vessel to be 10 knots an hour, and the engines to be making 36 revolutions a minute, then

$$\frac{v}{\omega}=4\tfrac{1}{2}\ \text{and}\ \left(2-\frac{v}{\omega r}\right)=0.$$

In this case the velocity of the vessel will be equal to the velo-

city of rotation of the edge of the blade nearest to the axis, and for one float

$$\begin{aligned} R &= \tfrac{1}{2}\rho_l\omega^2 r^4 \times \cdot 388103 \\ &= \tfrac{1}{2}\rho_l\omega^2\,(\text{radius})^4 \times \cdot 001516, \\ M &= \tfrac{1}{2}\rho_l\omega^2 r^5 \times 1\cdot 320900 \\ &= \tfrac{1}{2}\rho_l\omega^2\,(\text{radius})^5 \times \cdot 001290. \end{aligned}$$

Therefore the efficiency at full speed

$$\frac{Rv}{M\omega} = \frac{\cdot 001516}{\cdot 001290} \times \frac{v}{\omega\,(\text{rad.})} = \frac{\cdot 001516}{\cdot 001290} \times \frac{1}{2} = \cdot 588;$$

showing that a screw-surface which has the part nearest to the axis cut away is *not* so advantageous as a complete screw of the same size which has its axis at a greater distance from the centre of the paddle-wheel.

In consequence of the proximity of the paddle-wheel to the side of the vessel, it is found that the inner blades do not throw the water under the vessel, but against the side, so that it cannot escape; hence it has been found necessary to make all the screw-blades on the same wheel of the same kind, and to place them so as to throw the water outwards. As the pressure produced on the water is precisely the same with a left-handed as with a right-handed screw, it is quite clear that the results of the mathematical investigation will be equally trustworthy, whether the corresponding screw-blades be of the same or of opposite kinds.

LI. *On the Specific Resistance of the Metals in terms of the B. A. Unit* (1864) *of Electric Resistance, together with some Remarks on the so-called Mercury Unit. By* A. Matthiessen, *F.R.S., Lecturer on Chemistry in St. Mary's Hospital**.

As the B. A. unit will doubtless come into general use, it may not be out of place to give the resistance of the metals and some of the alloys in terms of it. Mr. Hockin and myself have determined †, in terms of this unit, the values of the resistance of certain lengths and weights of wires of several metals and one alloy; and when reduced to the same length and weight, namely, a metre long and gramme weight, we found their resistances to be, for hard-drawn wires,

* Communicated by the Author.

† Not yet published. These experiments were carried out for the Committee appointed by the British Association to report on Electrical Standards, and will appear in their Report of 1864.

Table I.

	Resistance in terms of B.A. unit (1864).
Copper	0·1469
Silver	0·1689
Gold	0·4150
Lead	2·257
Mercury	13·071
Gold-silver alloy . . .	1·668

These values are called by Professor William Thomson and Mr. Jenkin the specific resistances of the metals at 0° referred to unit of mass and length.

When carrying out the above determinations, we did not think of taking the specific gravity of the wires employed, as our object was only to see to what accuracy we could reproduce resistances by using wires of the above metals, &c. We employed the length weight in preference to the length section, knowing that the weight of a wire may be much more accurately determined than its section, whether deduced directly from the measurements of the diameter or indirectly from the specific gravity, the determination of the latter introducing a certain error. Of course, in endeavouring to reproduce resistances, it is wise to avoid the use of any unnecessary values; and it is just as well, and certainly a much more accurate method, to determine a resistance in length weight than in length section.

A strict comparison with former values will not be possible, owing to the above fact. Approximately, we may compare them by using the following specific gravities:—

Table II.

	Specific gravity.	Resistance in terms of the B. A. unit.	Conducting-power of hard-drawn wires of metre length and millimetre diameter.
Silver* . .	10·50	0·02048	48·83
Copper* .	8·95	0·02090	47·85
Gold* . .	19·27	0·02742	36·47
Lead* . .	11·391	0·2527	3·957
Mercury .	13·595	1·224	0·8172
Gold-silver†	15·218	0·1399	7·148

Or taking the conducting-power of the gold-silver alloy, 7·148 = 15·03, the following will be the values of the conducting-powers of the metals and some of their alloys:—

* Of cast specimens. † Of the wire.

TABLE III.

(Temperature of all, 0° C.*)

	Specific resistance in terms of the B. A. unit.			Specific conducting-power of metre length and millimetre diameter.		
	Hard-drawn.	Pressed.	Annealed.	Hard-drawn.	Pressed.	Annealed.
Silver	0·02103	.....	0·01937	47·56	.....	51·63
Copper	0·02104	.....	0·02057	47·53	.....	48·61
Gold	0·02697	.....	0·02650	37·08	.....	37·73
Aluminium	.....	.....	0·03751	.....	.....	26·66
Magnesium	.....	.....	0·05106	.....	.....	19·58
Zinc	.....	0·07244	.....	.....	13·80	
Cadmium	.....	0·08865	.....	.....	11·28	
Palladium	.....	.....	0·1140	.....	.....	8·77
Platinum	.....	.....	0·1166	.....	.....	8·58
Cobalt	.....	.....	0·1221	.....	.....	8·19
Iron	.....	.....	0·1251	.....	.....	7·99
Nickel	.....	.....	0·1604	.....	.....	6·23
Tin	.....	0·1701	.....	.....	5·88	
Thallium	.....	0·2296	.....	.....	4·36	
Lead	.....	0·2527	.....	.....	3·96	
Arsenic	.....	0·4417	.....	.....	2·27	
Antimony	.....	0·4571	.....	.....	2·19	
Bismuth	.....	1·689	.....	.....	0·591	
Mercury	.....	1·270		.....	0·788	
Platinum-silver alloy	.....	0·3140	.....	3·18		
German silver	.....	0·2695	.....	3·71		
Gold-silver alloy	.....	1·668	.....	7·148		

The platinum-silver alloy contains 66·6 per cent. silver, and is that alloy which has been used for making the copies of the B. A. unit†. On comparing these values with those deduced from the actual ones found, as given in the second Table, we find a difference with the silver of about 3 per cent. This is explained in our paper by the fact that the silver wire used was drawn in a peculiar manner; and on referring to another paper‡, like differences will be found in the conducting-power of silver. The differences in the other cases will be chiefly due to not using the correct specific gravities; thus, for instance, the specific gravity of copper used was 8·95. Siemens§ found it to vary for wire between 8·89 and 8·92; using the value 8·91, we find the conducting-power to be 47·64.

* Phil. Trans. 1864, p. 197, value for lead given in the Table is wrong; it ought to be 8·32, and not 8·23 (see Phil. Trans. 1862, p. 16).

† Report of the British Association, 1864. ‡ Phil. Trans. 1862, p. 6.

§ Poggendorff's *Annalen*, vol. cx. p. 18.

Again, the specific gravity of gold used is 19·27. The specific gravity given in Gmelin's 'Chemistry' for gold wire is 19·3 to 19·4. Using the value 19·4, we find the conducting-power to be 36·72. The differences between the values obtained for the mercury (namely, about 3 per cent.) show how difficult it is to obtain good results with this metal when only ordinary care is taken in experimenting. No doubt these differences are due to the manner of calibrating and deducing the values for the diameters of the tubes employed in my first experiments, published in 1858*; and when determining at a later period† the influence of temperature on its conducting-power (finding with the one tube we used nearly the same value), we did not think it necessary to repeat the determination. The value given by Mr. Hockin and myself is, as will presently be shown, probably the correct one.

In my papers on the conducting-power of metals and alloys, I have advocated the use of the gold-silver alloy as a ready and practical means, where only ordinary care is employed, of reproducing resistances. Here, again, we find my statements correct, for the values deduced by it agree well with the old ones, especially the lead one, a metal which has been recommended by Mr. Hockin and myself as eminently fit for the reproduction of resistances where great care is employed‡. Of course it is to be hoped that no one will try to reproduce any unit by means of this or any metal, now that the B. A. unit has been issued§; for they will certainly not obtain such accurate measures (without an *enormous trouble and expense*) as the copies of the B. A. unit at present issued by the Committee appointed by the British Association; and as proof of the correctness of this statement, I would draw the special attention of those who would endeavour to reproduce a unit, first, to what has been stated in the Reports of the Committee published in the Transactions of the British Association, and secondly, to the remarks I am now about to make on the sco-alled mercury unit.

It is defined as the resistance of a column of mercury a metre long and a square millimetre section at 0°. Now it may be asserted:—

1st. *That no true mercury unit has been issued.*

2nd. *That the units issued from time to time have not the same resistance.*

1st. *That no true mercury unit has been issued.* This assertion

* Phil. Trans. 1858, p. 383. † Ibid. 1862.

‡ The specific gravity of lead is said not to alter on being hammered or rolled (Reich, Journ. of Pract. Chem. vol. lxxviii. p. 730).

§ Copies of which may be obtained from the Secretary to the Committee (see Phil. Mag. vol. xxix. p. 248).

is based on the fact that for the calculations the wrong specific gravity of mercury has been used, viz. 13·557 at 0° instead of 13·595 as found by Regnault, Kopp, Balfour Stewart, and Neumann. These observers all give the same value for the specific gravity of mercury at 0° as compared with water at 4°. (Their values only vary up or down 0·007 per cent. from the above value, the maximum being 13·596, and the minimum being 13·594.)

These data prove either that no true mercury units have been issued, or if 13·557 be the true specific gravity of mercury at 0°, then the experiments of Regnault, Kopp, B. Stewart, and others must be all incorrect—a highly improbable result; in fact, one is irresistibly led to the conviction that it is utterly impossible that those distinguished observers can all have made such mistakes, and therefore that the value used, viz. 13·557, is wrong, and the units issued, supposing them to be otherwise quite correct, will each have a resistance about 0·3 per cent. greater than it ought to be.

2ndly. *That the units issued from time to time have not the same resistance.* In the Exhibition of 1861 were two sets of coils said to be reproductions of the mercury units; now the one set was exhibited by Siemens (London), the other by Siemens (Berlin), and when the two sets were compared with each other they showed a difference of about 1·2 per cent.

The value of the resistance of these two units in terms of the B. A. unit is as follows:—

Siemens (London)	0·9750
Siemens (Berlin)	0·9632

These values have been obtained in the following manner:—

Mr. F. Jenkin, in his Report on Electrical Instruments (Jurors' Report, 1862, p. 82), gives a Table containing the values of different coils, and on reference it will be seen that the coil whose resistance equals a mile of pure copper, &c., is equal to 13·95 Siemens (London) and 14·12 Siemens (Berlin) units. Now, when I adjusted this mile coil for Messrs. Elliots Brothers, I made another coil of the same resistance of different German-silver wire, and arranged it in a perfectly different manner from the coil tested by Mr. F. Jenkin. On comparing these two coils with each other after the lapse of three years, they were found exactly equal, showing that they had not altered; for it is hardly probable that two different specimens of wire put up in different methods should alter in resistance to a like extent.

Again, another proof of the mile coil not having altered is the following. Mr. F. Jenkin sends me the following value:—Thomson's coil (one in my possession, a duplicate of that sent to

Weber) was found by him in 1861 equal to 4·025 Siemens (London) units.

That Thomson's coil had not altered during this time was proved by comparing it again with a coil of wire of the gold-silver alloy which I made, and compared with the coils before they were sent to Weber in 1860. The new comparison agreed identically with the first one.

Thomson's coil = 4·025 S. (London) units = 15·543 of an arbitrarily chosen unit, made for my own experiments,
Mile coil . = 53·780 ditto;

and therefore the mile coil = 13·93 Siemens (London) units, proving that the mile coil has not altered, for Mr. Jenkin gives the value equal 13·95*, showing a difference of rather more than 0·15 per cent., a difference, considering the roundabout way the value is arrived at, which is very small; in fact it is much smaller than the differences of which I have now to speak.

Now the mile coil equals in resistance 13·59 B. A. units.

Mile coil equal 13·95 Siemens (London) units,
„ „ 14·12 „ (Berlin) units,

therefore

Siemens (London) unit . . . . . = 0·9750 B.A. units.
„ (Berlin) „ . . . . . = 0·9632 „
A new unit issued by the firm in 1864 . = 0·9564 „

It would be of interest to know which of these units Messrs. Siemens consider the correct one. Owing, however, to their using the wrong specific gravity for mercury, the values for comparison with others must be corrected by multiplying them by $\frac{13{\cdot}559}{13{\cdot}595}$, when we arrive at the following values for a metre of mercury of a square millimetre section at 0° according to

	True value in B. A. units.
Siemens (London) unit	0·9723
„ (Berlin) „	0·9605
„ (1864) „	0·9534
Mean	0·9620
Value deduced from experiments made by Mr. Hockin and myself (1864)	0·9619

From the above we see how closely the Siemens (Berlin) coil

* Mr. Jenkin informs me that the values given in his Table may possibly be 0·1 per cent. wrong; he does not think that any of them are 0·25 per cent. wrong.

agrees with the last value; and as the true value for the resistance of the mercury unit, as defined by Messrs. Siemens, we may take 0·961 B. A. units, a value differing from their 1864 issue by about 0·5 per cent., and when corrected for specific gravity, by about 0·8 per cent.

Now why do these differences exist? Are we not led to think from the papers written by these gentlemen, and others working in their laboratory, that the reproduction of the mercury unit is the most simple thing possible? I will quote what one of them says on the subject. Mr. R. Sabine states*, "Following this method, every electrician may inexpensively and with little trouble make himself a standard measure....The mercury unit has therefore been reproduced in Dr. Siemens's laboratory twenty-one times, six times in the first determination, five times in the second, and ten times in the present. And, allowing for the unfortunate misrepresentation of the measure by individual errors of the measuring-apparatus used in the comparison of the first tubes, the agreement between them all is greater than could be *guaranteed* between any two single electrical measurements with different measuring-apparatus. . . .From the foregoing results it follows that, by the method of direct production proposed by Dr. Siemens, much greater exactness has been attained than by means of any other of the methods of determination or copying."

I cannot allow the above passage to pass unnoticed, for it will be apt to mislead many who have not worked in this field of research. In the first place, we are told that this method of reproducing resistances is inexpensive and causes very little trouble. It may be inexpensive to those who possess first-rate apparatus for normal weighings or measurements, normal thermometers, &c.; for with first-rate apparatus only normal results can be obtained. Then, as to the trouble; does anybody for a moment suppose that a standard measure can be reproduced with a *little trouble*? Would it not take any observer weeks to reproduce with accuracy a resistance which can be relied on? must not such an observer check and recheck every determination he makes? must not he be sure all his instruments are graduated accurately? and he can only be sure of this if they are carefully tested by himself, or by perfectly reliable persons; and, what is of very great importance, must he not be sure that he deals with an absolutely pure metal? Did not Mr. C. W. Siemens himself state at the meeting of the Royal Society when remarks had been made upon Mr. Jenkin's paper on the B. A. unit, that he was loth to give up the use of the mercury unit, as he was afraid the B. A. unit could not be accurately reproduced,

* Phil. Mag. vol. xxv. p. 172 (1863).

for experience in reproducing the mercury unit had taught him the great difficulty of such operations? I think Mr. Sabine's sentence that I refer to would have been more correct if thus written:—Following this method, every electrician may, with perfect apparatus, &c., and with a very great deal of trouble, by taking some weeks for his determinations, together with the use of all precautions to ensure normal results, as well as employing proper constants and coefficients for deducing and correcting the values so obtained, make himself a standard measure.

In the second place, we are told that the unit has been reproduced twenty-one times, and that, allowing for the unfortunate misrepresentation of the measure by individual errors of the apparatus used in the comparison of the first tubes, the agreement between them all is greater than could be guaranteed between any two single measurements with different measuring-apparatus.

The first tubes show, as I have elsewhere pointed out, a maximum difference between the calculated and observed resistances of 1·6 per cent., and this difference is said to be caused by errors in the measuring-apparatus. Assuming this to be the case, it proves that the precautions to be adopted in adjusting the apparatus must be very great. For if in Dr. Siemens's hands, with his skill and with such good apparatus as is described in his paper, such errors can occur in his endeavours to reproduce this unit, how much more are errors to be feared from others who have not such good apparatus at their command! As to the agreement between the other reproductions being more accurate than any two measurements with different apparatus, the following facts will constitute a good answer.

1. The Messrs. Siemens's reproductions, they state, agree within 0·05 per cent. Two coils* were compared with each other some years ago (the one German silver, the other gold silver), and now, with different apparatus and by a different observer, they were again compared: the ratio of their resistances was found absolutely the same.

2. The Messrs. Siemens have issued an 1864 mercury unit. Now four different copies of this have been compared with four different copies of the B. A. unit with four different measuring-apparatus, and by four different observers †, and the ratios of the resistances, as determined by the four observers, do not vary between each other more than 0·02 per cent.

In the third place, we are told that, by direct production, much greater accuracy has been attained than by any other method of determination or copying.

* See Report of Electrical Standard Committee, 1864.

† See Report on the B. A. Unit, by F. Jenkin, Proc. Roy. Soc. No. 74.

Copies of a unit may be made, I maintain, to any accuracy required: all the B. A. units agree together, when issued, to within 0·01 per cent., of course at the temperature stated on them: they are all kept a month after making; if during that time they alter in the least, they are taken to pieces and remade; out of fifty already made, only two have had to be altered, the cause of failure being in both cases faulty soldering.

This statement, together with the above fact of the four different copies of the 1864 mercury unit agreeing so well together, prove that copies of a unit can be, and are, made to agree together much more accurately than it can be reproduced even when great care is taken.

If the reproduction of a unit be so easy a matter, how are we to explain the large differences observed in the copies of the different issues of the mercury unit. The simple truth is that the reproduction of any unit, where great accuracy is required, is exceedingly difficult. Take, for instance, the reproduction of the normal pound by Professor Miller of Cambridge: one would think, at first sight, nothing easier; but when we enter into the minor details, we find how difficult the problem was to solve.

It would have been better if, instead of reproducing the unit from time to time, Messrs. Siemens had done what the Committee on Electrical Standards appointed by the British Association have done—namely, first determined the value of their unit as accurately as possible, and then made copies of it in several different materials. It is not possible that all these copies will alter to a like extent by age, and it is very probable that most of them will not alter at all: they will, of course, be tested from time to time; and at the Kew Observatory, where they will be deposited, arrangements will be shortly made for comparing also, from time to time, at a small cost, the coils issued by the Committee, thus offering a ready means of ascertaining whether the coils issued are constant or not. The coils issued by the Committee are copies of the B. A. unit, and at the temperature marked on them are correct to within 0·01 per cent. By using these coils, the results of different observers become comparable. The Committee of Electrical Standards do not wish to imply that the resistance of their unit is exactly equal to $10^7 \frac{\text{metre}}{\text{second}}$ Weber's electromagnetic units, but only a close approximation to it. If in a few years' time new and more accurate determinations be made, then, supposing a correction required, the copies of the B. A. unit (1864) being all exactly equal to one another, will have a small value to be added to or subtracted from their resistance, and all the comparisons made with them corrected by the same coefficient.

Had the Messrs. Siemens followed this plan, then the use of the wrong specific gravity would not so much influence the value of their coils as means of comparison, as the differences in the values of the coils themselves; for when we find a Siemens unit used, we must first discover what value it has, because, as already pointed out, the different issues have different values.

LII. *On some Electrical Experiments.*

By J. J. Waterston, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

IN § 30 of the paper on Electric Experiments, I have inadvertently stated that the leaves of the electroscope diverged with negative electricity, instead of positive. The experiment was meant to direct attention to a simple mode of proving, by means of a Leyden jar and electroscope, that the force of electric induction is capable of acting in *curved lines*, a fact which appears to deserve that distinct announcement due to a fundamental characteristic of the electric force.

Suppose we take a coated plate of glass properly insulated, and charge it highly with positive electricity on the side A, and negative on the opposite side B. After leaving it in this charged state for a few seconds, let an electroscope be brought with its cap near the centre of A, but not touching it: the leaves are found to diverge with positive in consequence of the free positive on the outer side of coating A acting inductively on the cap of electroscope, exciting upon it negative, and on the gold-leaves a corresponding amount of positive. In the same way, if we remove the electroscope from near A and place it on the opposite side close to centre of B, the leaves will diverge with negative from the action of the free negative on the outer side of B.

While the electroscope is thus stationed close to B, let A be touched: the leaves increase their divergence, showing that the free negative on the outer side of B has received an augmentation. The electroscope is then to be immediately removed to the opposite side, and the cap made to touch the centre of coating A. The leaves do not open, there is no longer electric excitement on the *outer* side of A: it has been discharged by touching. If we gradually remove the electroscope further away from the plate, but still keep it opposite the centre of A, the leaves diverge with *negative* more and more until a certain maximum is attained, after which they gradually sink as the distance is further increased. The inductive action cannot come from any part of the adjacent coating A, which is altogether positive; it

must therefore be supplied from B, the free negative on the outer side of which thus acts not only on the B side of the plate, but on the A side also, *to attain which it must pass round the edges of the coating* A.

Faraday supposes this to take place by means of the action of contiguous particles of air; and hence it is to be presumed that it is his belief that a charged pane could not exist in an illimitable vacuum, or at least that the electricity on B could not affect an electroscope on the side of A, because there would be no contiguous particles to transmit the force.

This hypothesis has been supposed to be further supported by the apparent augmentation in the force of induction that takes place when a plate of lac or sulphur is interposed between the charged body and a neighbouring conductor; and the term *specific inductive capacity*, applied to denote the degree of the special faculty of transmitting the inductive power belonging to the particles of a given non-conducting body, seems to be now established, Harris having published experiments as supporting Faraday's views, and teaching them in his elementary treatise.

As this term appears to me inapplicable to matter in relation to electric phenomena, I beg to submit the following experiment as demonstrating the non-existence of such a quality.

An insulated brass disk (the B. D. of previous experiments) is suspended over the cap of the electroscope at the distance of 2 inches above it. A cake of sulphur, 4 inches in diameter and $\frac{1}{2}$ an inch thick, is provided with handles of sealing-wax projecting about 10 inches from each side. It is examined to see if there is any excitement upon A; and if any, discharge is effected by presenting it to a flame. The B. D. is charged from an electrophorus so that the leaves of the electroscope diverge to about 60°. The cake of sulphur, supported by its handles (which enable the hands of the operator to be kept at such a distance as not to influence the electroscope), is brought horizontally into the space between the B. D. and cap of the electroscope. It is then moved parallel up and down; now close to the cap, then close to B. D. When close to B. D., the leaves are not at a higher angle than before its introduction; but on bringing it down towards the cap the angle increases a little, and when quite close to it the angle increases considerably, and this notable augmentation takes place within a distance of about $\frac{1}{4}$ of an inch from the cap. The experiment requires to be promptly performed, because the sulphur soon becomes inductively excited in the manner peculiar to non-conductors and as, to a certain degree, developed in the previous experiments. On first bringing the cake between the B. D. and the electroscope, the leaves descend a little just as the edge of the cake passes in between the outer

edges of the B. D. and cap. Next let us take a metallic disk of the same diameter as the cake, and having also applied similar insulating handles, introduce it and move it up and down between the B. D. and cap without touching either. The leaves of the electroscope will be found to be acted upon in the same way as before, but in a greater degree.

I submit that if the sulphur had a greater capacity than air of transmitting induction, the leaves of the electroscope ought to rise higher when the cake is close to the charged B. D. than when it is close to the cap of the electroscope. In the latter case the increase of the angle of the gold-leaves was fully as great as when the electroscope was brought half an inch nearer to the B. D. In the former case no increase of induction was apparent.

The same experiment, repeated with sealing-wax and gutta-percha plates, showed similar results, but more striking. When close to B. D. the inductive force was less than through air; when close to the cap it was greater. [Gutta percha shows some curious abnormal effects, especially when heated and soft. Although it is then still a perfect non-conductor, it seems to absorb the inductive force like a sponge.] A change in the shape or size of the non-conducting plates modifies the result, exactly in the same manner as the conducting plates, though always in a less degree. The proper way of studying the subject would therefore seem to be to experiment with conducting and non-conducting plates of the same size, and guard against the effect of induction upon the non-conducting surfaces, which seems to be a phenomenon of a special kind, that stands in need of further elucidation. I have not been able to arrive at any distinct law of action.

The transition from one kind of induction to the other may be conveniently studied in a warm room during dry weather, by placing a shade of thin glass over the electroscope, of such size that the round top may be about an inch above the cap, the agent being an insulated brass disk (the B. D.) charged inductively from an excited plate of sulphur. If we moisten the outside of the shade with a sponge and bring the excited B. D. close to the top, but not touching, there is of course no effect on the electroscope, the surface is opake to the electric force; the induction is confined to the aqueous surface opposite the B. D. As the shade dries we observe a slight effect on the gold-leaves, which becomes more and more apparent as the glass resumes the dielectric condition. Let the shade be now heated before a clear fire, and replaced, when cool, over the electroscope. We may now study the effect of moving the excited B. D. to and from the top of the shade. If the action is performed without pause, the leaves open and shut in the regular normal way; but

if the B. D., when close to the top of the shade, is kept in position for a few seconds, the leaves sink a little (showing a decrement $\triangle$ of the inductive force acting on the electroscope, although there is no decrement of the charge on B. D.); and on removing it they shut and then open with the opposite electricity, and the extent of the opening corresponds to the decrement $\triangle$, thus showing that the top of the glass shade has been inductively excited by B. D. But this excitement is evanescent, the leaves subside in a few seconds. The explanation of such phenomena that first presents itself is, that the outer surface of the shade is a partial conductor; but if this were the case, how is it that on bringing the B. D. into contact with the glass shade it does not lose charge, although its intensity is much greater than the induced excitement which so quickly passes away? The lesser intensity cannot be conducted more easily than the greater. Insulating the glass shade, by resting it upon sulphur, makes no difference. It appears to be a constrained local polarization, in which the work-equivalent of the charge is partially expended in effecting a change of molecular condition; but there is no separation of the electricities accompanied with conduction, as takes place on a truly conducting metallic surface subject to the inductive influence of a charged body.

Faraday's view of static induction has led him to give a definition of disruptive discharge, which I think is also unfortunate as tending to close further inquiry in the very direction most likely to lead to important discovery in the dynamical conditions affecting the relations of molecules to the higher potential medium. In recent educational works it is mentioned, not as a conjecture, but as an established fact, that "the particles of the intervening dielectric *are* brought up to a highly polarized state, until at length the tension on one particle rising higher than the rest, and exceeding that which it can sustain, it breaks down; the balance of induction is thus destroyed, and the discharge is completed in the line of least resistance." In my tenth and eleventh experiments it may be remarked that the disruption that accompanies discharge is like what is caused by mechanical impulse of matter pressing with sudden and short-lived force. If this idea is kept in view and arrangements made in accordance with it, experiments on the discharge of high-tension electricity would, I believe, assume a new and exciting interest, and lead, if well planned, to a deeper insight into the arrangement of force in nature than we could otherwise hope for.

I remain, Gentlemen,

Your obedient Servant,

JOHN JAMES WATERSTON.

Edinburgh, March 20, 1865.

LIII. *Chemical Notices from Foreign Journals.* By E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 313.]

WITH a view to finding an effectual method of separating from each other Mosander's three bases, yttria, erbia, and terbia, Popp undertook an investigation of yttria. He has published the results of his investigation separately, and has given a summary of it in Liebig's *Annalen**, from which the following is taken.

As distinct oxides he considers that erbia and terbia have no separate existence, or rather they are not the oxides of hitherto unknown metals, but are identical with the oxides of cerium and didymium. Mosander's oxide of yttria contains both alkali and lime. Hence erbium and terbium, which have heretofore been regarded as elements, though not altogether without some doubt, must now be struck from the list.

Pure yttria is separated from its accompanying earths by means of carbonate of baryta, which completely precipitates oxides of cerium and didymium even in the cold, but not yttria. It is necessary that the cerium be present as sesquioxide. When freshly precipitated, yttria closely resembles hydrate of alumina, and is of a pure white colour. A tinge of red indicates that it contains oxide of cerium. Ignited, it is a heavy yellowish-white powder, which, if quite white, contains alkali and lime. Hydrated oxide of yttria corresponds approximately to the formula YO, 2HO. It is a strong base, and expels ammonia from its salts when boiled. In its relations to the alkalies it shows close analogies with magnesia.

All the yttria salts have a distinctly light rose-colour, which is not caused by the presence of manganese or didymium, but is peculiar to them, and which they lose when they are dehydrated.

It is characteristic of the yttria salts that when a solution is interposed between the prism of a spectroscope and a strongly luminous gas-flame, five black lines are seen, which are quite different, both in number, position, and intensity, from the similar lines of didymium. One of these lines is in the deep violet, and the other in the extreme red.

The combining proportion of yttrium was determined afresh, inasmuch as Berzelius, who found the number 40, had evidently worked with impure yttria. Popp used the sulphate which separates from a boiling solution of sulphate of yttria, and which Berzelius had employed. The yttria was precipitated by means of oxalic acid, and the oxalate ignited and weighed. From the

* August 1864.

relation of the sulphuric acid to the yttria found, Popp deduces the number 42 as the combining proportion of yttria, which, assuming that it has the composition YO, gives 31 for the combining proportion of yttrium.

The metal yttrium was reduced from the chloride of ammonium and yttrium by means of sodium, in a manner analogous to that by which magnesium is prepared. While moist it is a black glittering powder, which, while drying, becomes lighter, owing to oxidation. When quite dry it does not oxidize in the air; at ordinary temperatures it only slowly oxidizes in water; on boiling, the oxidation is more rapid, but even then is not complete. It is readily dissolved by dilute acids with disengagement of hydrogen.

Heated on platinum foil, yttrium burns with intense lustre; in pure oxygen the combustion is most brilliant; the colour is not of such a pure white as is that of magnesium and aluminium; it inclines towards reddish.

By the properties of the metal, as well as those of its compounds, yttrium is allied to magnesium; and by its property of being precipitated by ammonia it is the connecting link between the alkaline earths and the earths proper. Some of the best-defined and most important of its salts are the following.

The *sulphide of yttrium* is difficult to prepare. The best results are obtained by passing dry bisulphide-of-carbon vapour mixed with dry hydrogen over heated yttria. *Chloride of yttrium*, $YCl + 6HO$, crystallizes in rhombic very deliquescent plates. Chloride of yttrium is non-volatile and a chloro-base; it forms with chloride of mercury easily soluble deliquescent crystals, which have the formula $YCl, 2HgCl + 6HO$. *Sulphate of yttria*, $2YOSO^3 + 5HO$, has the peculiarity that its solubility diminishes proportionally to the increase of temperature. At 30° to 40° its solution becomes turbid, and on boiling, all the salt separates out as a heavy crystalline powder. *Nitrate of yttria*, dissolved in alcohol and evaporated over alcohol, forms well-defined rhombic plates, which are deliquescent and have the composition $YO\,NO^5 + 3HO$. *Carbonate of yttria*, $YO\,CO^2 + 3HO$, is obtained, by precipitating a solution of yttrium with carbonate of sodium, as a gelatinous precipitate, which, digested with an excess of carbonate of soda, is converted into small white crystalline needles. The *acetate of yttria* is the most easily crystallizable of all the salts; it has the composition $YO\,\dot{A}c + 2HO$.

Redtenbacher has described to the Vienna Academy of Sciences* an improved method of separating the metals potassium, rubidium,

* *Bulletin*, No. 8. 1865.

and cæsium, which depends on the solubility of the alums of these three bases.

All three alums are easily soluble in hot water; but while 100 parts of water at 17° C. dissolve 13·5 of potash-alum, they dissolve 2·27 of rubidium-, and 0·619 of cæsium-alum. Hence at 17°C. potash-alum is thirteen times, rubidium-alum fifteen times, and cæsium-alum eighty-eight times as soluble as the corresponding platinum-salts. And while the solubility of the alums in the different cases is as

Potassium.		Rubidium.		Cæsium.
22	:	4	:	1,

that of the platinum-salts is

15	:	2	:	1.

By this deportment a way is opened for the separation and preparation of these metals on a large scale.

Schiel describes* the following as a lecture experiment. A test-tube containing peroxide of silver, drawn out in the middle and sealed, is introduced into a well-dried stoppered gas-bottle, which is then filled with chlorine. The flask being closed, the tube is broken by shaking; the chlorine becomes paler, and in a few minutes the flask is full of colourless oxygen gas. The proportions of peroxide and chlorine must be those of their equivalents. The peroxide is readily prepared by passing ozonized oxygen over dry oxide of silver.

Popp† designates the following as the best of two methods which he gives for separating oxide of cerium from the oxides of lanthanum and didymium, with which it is usually associated. The three oxides, dissolved in acid, are approximately neutralized without producing a permanent precipitate. An adequate quantity of acetate of soda is added and then an excess of hypochlorite of soda, and the whole boiled for some time. The cerium is deposited as a bright-yellow precipitate, the other bodies remain in solution. The filtrate must not become turbid on the addition of hypochlorite and boiling; by careful and sufficient addition of hypochlorite in the first case, one treatment is sufficient, and every trace of cerium is precipitated free from lanthanum and didymium.

The yellow precipitate contains much water; it dries to a brownish-yellow, transparent and easily pulverizable mass. It has all the characters of a peroxide, and is doubtless CeO^2; it is formed in the same way as the peroxides of lead and manga-

* Liebig's *Annalen*, December 1864.
† Ibid. September 1864.

nese. It dissolves in hydrochloric acid with disengagement of chlorine, yielding a solution of a clear saffron-yellow colour, which diminishes on dilution, and disappears completely on boiling. Nitric and sulphuric acids dissolve the peroxide with a reddish-yellow colour, forming corresponding compounds of the oxide.

Popp finds that the peroxides of nickel and cobalt are formed in the same manner as that of cerium.

Marignac* has found that when a solution of acid tungstate of potash or soda is boiled with gelatinous silicic acid, the liquid assumes an alkaline reaction, and is now found to contain a new acid in which 1 equiv. of silica is united with 12 equivs. of tungstic acid, $SiO^2\,12WO^3$, which he calls *silico-tungstic acid*. It is a powerful acid, which is easily separated from its salts, forms two splendidly crystallized hydrates, and salts which for the most part are soluble and crystallize well.

Under the same circumstances as above, acid tungstate of ammonia gives rise to another acid in which an equivalent of silica is united with ten of tungstic acid, $SiO^2, 10WO^3$, and which Marignac names *silico-decatungstic acid*. It is more difficult to obtain pure; its hydrate does not crystallize; and from the very soluble character of its salts, it is difficult to obtain them in crystals.

The acid itself is not very stable; when heated, a small quantity of silica separates out, and a new acid is obtained containing silica and tungstic acid in the same proportions as in silico-tungstic acid, but which in other respects is totally different, and which has accordingly been named *tungsto-silicic acid*. It forms a soluble and deliquescent hydrate, which can, however, be obtained in well-formed crystals. It forms a series of salts isomeric with the silico-tungstates, but differing from them in crystalline form, and in the amount of water of crystallization, and also in being somewhat more soluble.

All these acids are quadribasic, if the salt with 4 equivs. base is to be taken as neutral, and are formed when the acids act upon the carbonates of a base. The most frequent salts are those with 2 or 4 equivs. base; the first crystallize most easily. They have a great tendency to form double salts; hence it is that ammonia does not precipitate a solution of silico-tungstate of alumina, and that alumina, magnesia, carbonate of lime, &c. are dissolved by silico-tungstate of ammonia. As the salts of the acids are easily soluble, and contain a large quantity of tungstic acid, their solutions have a remarkably high specific gravity. The solution of silico-tungstate of soda has the specific gravity 3·05, so that glass, quartz, and most rocks float upon this, which is moreover a very mobile liquid.

* *Comptes Rendus*, vol. lviii. p. 809.

Silico-tungstic acid crystallizes at ordinary temperature in thick quadratic pyramids, and which contain 29 equivs. water of crystallization, according to the formula

$$SiO^2\,12WO^3\,4HO + 29Aq:$$

if it crystallizes at somewhat higher temperature, or at ordinary temperature in the presence of alcohol, hydrochloric acid, or sulphuric acid, it forms a hydrate which only contains 18 equivs. water of crystallization, and of a different crystalline form. At 100° the acid retains, besides the basic water, 4 equivs. of water of crystallization. On heating at 220° it loses the latter, but retains 2 equivs. of basic water: these appear necessary for the constitution of the acid; they are only removed at the temperature of red heat.

Tungsto-silicic acid crystallizes with 20 equivs. of water of crystallization, and has the formula

$$12WO^3, SiO^2\,4HO + 20Aq.$$

It is acted upon by heat in the same way as silico-tungstic acid.

Marignac has made a crystallographic investigation of the salts of these acids; and although relations to other compounds have not been found (since these acids appear to belong to a new type of combination), they have furnished a number of interesting results. These, as well as reasons against Persoz's proposal to change the formula of tungstic acid, Marignac discusses at length.

Weltzien* describes a method for the quantitative estimation of nitric acid in waters. It consists in determining it as nitrogen gas. The water to be investigated is evaporated, and by a careful addition of carbonate of soda, the lime and magnesia are precipitated as carbonates, and any nitrate of lime or magnesia is converted into nitrate of soda. The filtrate from this precipitate is evaporated to dryness, and mixed with finely divided copper reduced from the oxide by hydrogen. This mixture is heated in a somewhat long combustion-tube in the usual mode of determining nitrogen, all the atmospheric air having been previously expelled by carbonic acid. Weltzien gives a number of analyses of the waters of the town of Carlsruhe investigated by this method, and also, to show its accuracy, determinations made with known quantities of nitrates. His analyses of the town well-waters show in a marked degree the influence of population in increasing the quantity of nitrates. They show also the completeness with which any nitrogenous organic matter becomes oxidized into nitrates.

* Liebig's *Annalen*, November 1864.

According to Lamy*, phosphoric acid forms with thallium a series of phosphates, for the most part very soluble, and varied in their composition and properties like the corresponding compounds of the alkaline metals. They are as follows:—

Neutral phosphate of thallium	$PO^5, 2TlO_{,,} HO + HO$
Acid „ „	$PO^5, TlO, 2HO$
Basic „ „	$PO^5, 3TlO$
Neutral pyrophosphate „	$PO^5, 2TlO$
Acid pyrophosphate „	PO^5, TlO, HO
Metaphosphate „	PO^5, TlO.

All these salts are white, and almost all soluble in water and insoluble in alcohol. They are distinguished from the alkaline phosphates by the fact that they give a white precipitate with hydrochloric acid, and also with nitric acid, provided their solutions are neither too hot nor too dilute. The phosphates and pyrophosphates give a white precipitate of tribasic phosphate with the alkalies, while they are not precipitated by alkaline carbonates, nor even by alkalies in the presence of these carbonates.

After a description of these phosphates, M. Lamy offers some considerations on the position of thallium. In previous researches on thallium he had classed this metal by the side of the alkaline metals, in which he had the countenance of M. Dumas. Mr. Crookes, on the other hand, places thallium near the heavy metals—principally on account of the insolubility of some compounds, such as the peroxide, the protochloride, the iodide, sulphate, and phosphate of thallium, the facility with which the protoxide is dehydrated and loses most of its solubility, the high atomic weight of the metal, the ready reduction of its salts by zinc, and generally most of its physical properties.

M. Lamy, while admitting the insolubility of its bromide, &c., points out that thallium forms soluble higher chlorides, a soluble fluoride, and a soluble double fluoride with silicium. He thinks the physical properties of secondary importance, but principally relies in support of his view on the following considerations.

The hydrated protoxide of thallium is very soluble in water, strongly alkaline and caustic like potash; its carbonate is soluble and alkaline like that of potash; there are phosphates and arseniates various in composition and properties, like the corresponding alkaline compounds; the sulphate is soluble, and has most of the properties of sulphate of potash, with which it is isomorphous; there is analogy of properties and isomorphism in the case of the thallium and potassium alums; and this analogy is also met with in the case of the double sulphates of the magnesia

* *Comptes Rendus*, April 10, 1865.

series, and in the paratartrates and bitartrates. Like the alkalies, it forms double salts, the number of which increases the more this curious metal is studied. It forms neither subnitrate nor subacetate; but its acetate, distilled with arsenious acid, furnishes cacodyle. Lastly, thallium has, like the alkaline metals, and them alone, the characteristic property of forming thallic alcohols.

M. Lorin* describes a mode of reduction which is an application of the following property. An ammoniacal salt, a simple or a compound base, gives in general, in the presence of zinc and water, a disengagement of hydrogen—frequently at the ordinary temperature, but better towards 40° and upwards.

This property has been verified on about fifty ordinary salts of ammonia, and on a small number of salts of methylamine, ethylamine, aniline, and naphthylamine.

The quantity of hydrogen appears to be a function of the equivalent of the acid. An equivalent of sulphate of ammonia gave at least an equivalent of hydrogen.

Of the ordinary metals, iron is the only one which approaches zinc in its action on ammoniacal salts, though it is less intense.

The concurrence of zinc, iron, ammonia, and an ammoniacal salt are the best conditions for accelerating the production of hydrogen. The rapidity of the disengagement is almost equal to that for dilute acids.

One exception is met with in the case of nitrate of ammonia, which on dilute solution gives protoxide of nitrogen at a temperature near 50°.

LIV. *On the Equilibrium of Arched Ribs of Uniform Section. By* James H. Cotterill, *B.A., Scholar of St. John's College, Cambridge*†.

1. THE object of this article is to show how to find approximately the thrust, shear, and bending moment on any section of an arched rib acted on by given forces under given circumstances; the method is founded on the principle of Least Action, which in a former article was employed in the case of straight beams.

Let transverse planes be drawn indefinitely near to each other, meeting in the centre of curvature at any point, cutting off a small portion of the rib. Let the forces which may be acting on the rib, whether distributed or detached, be supposed to act at points in the line joining the centres of gravity of its trans-

* *Comptes Rendus*, April 10, 1865.

† Communicated by the Author.

verse sections, or, as it will be called hereafter, the axis of the rib; it is plain that this supposition, which is exactly true for the weight of the rib, will in other cases produce no considerable error. Also let the stress on any section of the rib be resolved into a shear F, a thrust H, and a bending moment M; and let it moreover be supposed that the shear and thrust may be considered as concentrated at the centre of gravity of the section. These suppositions being made, we have for equilibrium of the small portion, by resolving tangentially and normally and taking moments,

$$H d\phi = dF + p ds,$$

$$F d\phi + dH = q ds,$$

$$dM = F \,.\, ds,$$

where $d\phi$ is an element of the angle which the section considered makes with a given section; ds an element of the axis; p the normal pressure estimated per unit of length of the axis and reckoned positive when acting inwards; q the tangential pressure per unit of length of the axis, reckoned positive when tending to increase ϕ. If ρ be the radius of curvature of the axis, these equations may be written,

$$\frac{dF}{d\phi} + p\rho = H,$$

$$\frac{dH}{d\phi} + F = q\rho,$$

$$\frac{dM}{d\phi} = F\rho.$$

Differentiating the first equation and adding it to the second, we have

$$\frac{d^2F}{d\phi^2} + F = q\rho - \frac{d}{d\phi}\{p\rho\},$$

whence

$$F = K + F_0 \,.\, \cos\phi + H_0 \,.\, \sin\phi,$$

where F_0, H_0 are arbitrary constants,

$$K = \left\{1 + \frac{d^2}{d\phi^2}\right\}^{-1}\left\{q\rho - \frac{d}{d\phi}\{p\rho\}\right\},$$

$$H = \frac{dF}{d\phi} + p\rho = p\rho + \frac{dK}{d\phi} + H_0 \,.\, \cos\phi - F_0 \,.\, \sin\phi,$$

and

$$\frac{dM}{d\phi} = F \cdot \rho = K\rho + F_0 \cdot \rho \cdot \cos\phi + H_0 \cdot \rho \cdot \sin\phi;$$

$$\therefore M = \int_0^\phi K\rho d\phi + F_0 \cdot \int_0^\phi \rho \cdot \cos\phi d\phi + H_0 \int_0^\phi \rho \cdot \sin\phi d\phi + M_0.$$

Thus M, H, and F are expressed in terms of known functions of ϕ, and three undetermined constants which, if no distributed forces act on the rib, are the shear, thrust, and bending moment on the initial section. In these expressions shear is reckoned positive when it tends to draw away from the centre of curvature that portion of the rib which is *towards the algebraical increase of* ϕ, thrust when it tends to move the same portion towards the increase of ϕ, and bending moment when tending to twist the same portion in the direction of ϕ.

Now it may be that something is known about the stress on one or more sections, and in that case one or more of the constants will be definite; but in general it is impossible to determine them without a knowledge of the physical constitution of the rib, because their values entirely depend on that constitution. But if the nature of the material be known, so that the work done in it by the action of the forces can be estimated, then will the actual values of the constants be such as to make that work the least possible, and the differential coefficients of that work with respect to the constants will furnish the means of finding their values. I proceed, therefore, to find the general values of those coefficients, supposing that the rib is composed of homogeneous material strained within the limits of perfect elasticity.

Before doing so, however, I may remark that it is frequently convenient to use rectangular equations of equilibrium; the rib is to be divided into plates by planes parallel to the axis, and differential equations obtained expressing the equilibrium of one of those plates*.

2. When a straight beam is subjected to a bending moment, the strain being supposed within the limits of perfect elasticity, then it is known that the stress at different points of any transverse section varies uniformly, vanishing at points situated in a straight line, through the centre of gravity of the section, parallel to the axis of the applied couple.

In effect, considerations of symmetry show that any particles originally in two transverse planes will, after application of the

* It is worth remarking that $q\rho = \frac{d}{d\phi}\{p\rho\}$ is the equation of a linear arch.

bending moment, still be in two transverse planes, whence the geometry of the question shows that the elongation and shortening of lines in the beam parallel to its axis will be proportional to their distances from some line situate in a transverse section, and that consequently the stress must vary uniformly; and since, moreover, the total amount of the stress is a couple, the line of no stress or neutral axis must pass through the centre of gravity of the section.

Now, if the beam be curved, the same considerations of symmetry apply to show that particles originally in two transverse sections will still be in two transverse sections; but since those transverse sections are not parallel in their original positions, the geometry of the question is different, and consequently the stress does not vary uniformly, nor does the neutral axis pass through the centre of gravity of the section. By the method indicated, the law of variation of the stress, and the deviation of the neutral axis from the centre of gravity, might be determined, and also the maximum stress on the section. But it is probable that the introduction of terms depending on the curvature of the beam would not add materially to the accuracy of the results; for in general not only does a bending couple act on the beam, but also forces which cause a shear on most of its sections; and the accuracy of the above conclusions is thereby disturbed to an unknown though (from the values of the coefficients of direct and transverse elasticity of the materials used in construction) small extent; and this error, in cases where the curvature is moderate in proportion to the depth, may probably be as great as the error produced by neglecting the curvature of the beam. We may take therefore

$$U = \int \left\{ \frac{M^2}{2EI} + \frac{H^2}{2EA} \right\} ds$$

as a first approximation to the work done. In cases in which the angle subtended by the rib is considerable, and in which, moreover, the distribution of the load differs much from that which is necessary for the equilibrium of a linear arch of like form, then, the thrust being small, the work done is almost entirely done by the bending moment, and a sufficient approximation is given by

$$U = \int \frac{M^2}{2EI} ds = \int \frac{M^2 \rho}{2EI} d\phi.$$

Reference will be subsequently made to cases in which these conditions are not satisfied.

Using this formula, if the value of M given above were substituted and the result integrated throughout the beam, the result

would oe the potential, by differentiation of which the values of the constants could be determined; but inasmuch as this function is of great complexity, it is necessary to proceed otherwise, and calculate the several differential coefficients separately. By differentiation,

$$\frac{dU}{dF_0} = \int \frac{M\rho}{EI} \frac{dM}{dF_0} d\phi = \int \frac{M\rho}{EI} \int_0^\phi \rho \,.\, \cos\phi \,.\, d\phi \, d\phi ;$$

so that if we merge the factor EI in U,

$$\frac{dU}{dF_0} = \int M\rho \int_0^\phi \rho \,.\, \cos\phi \,.\, d\phi \, d\phi ;$$

so also

$$\frac{dU}{dH_0} = \int M\rho \int_0^\phi \rho \,.\, \sin\phi \,.\, d\phi \,.\, d\phi,$$

$$\frac{dU}{dM_0} = \int M\rho \,.\, d\phi.$$

In calculating these coefficients for a given form of beam, it is convenient to observe that they consist of two parts—one due to the arbitrary constants, and the same for every load, and the other due to the mode of distribution of the load. If the beam be acted on by detached forces, then the portion due to the constants must be estimated afresh for every part of the rib lying between those detached forces; but the portion of the coefficients due to the distributed load may be taken throughout the rib, without reference to any detached forces which may act upon it*.

3. I proceed to calculate the values of the parts of the coefficients due to the arbitrary constants in the case of a circular rib of radius r. Here $\rho = r$, and consequently the coefficients are

$$\frac{dU}{dF_0} = r^2 \int_0^\phi M \,.\, \sin\phi \,.\, d\phi ; \qquad \frac{dU}{dH_0} = r^2 \int_0^\phi M(1 - \cos\phi).d\phi ;$$

$$\frac{dU}{dM_0} = r \int_0^\phi M d\phi ;$$

and so far as the arbitrary constants are concerned,

$$M = M_0 + F_0 \,.\, r \,.\, \sin\phi + H_0 r(1 - \cos\phi).$$

On substitution and integration, we find

* It is of course supposed that the change of form of the rib is inconsiderable; otherwise a preliminary question would have to be solved—namely, to find ρ as a function of ϕ by means of the calculus of variations.

$$\frac{dU}{dF_0} = F_0 r^3 \{\tfrac{1}{2}\phi - \tfrac{1}{4}\sin 2\phi\} + H_0 r^3 \{\tfrac{1}{4}.\cos 2\phi - \cos\phi - \tfrac{3}{4}\} + M_0 r^2 \{1 - \cos\phi\},$$

$$\frac{dU}{dH_0} = F_0 r^3 \{\tfrac{3}{4} - \cos\phi + \tfrac{1}{4}\cos 2\phi\} + H_0 r^3 \{\tfrac{3}{2}\phi - 2.\sin\phi + \tfrac{1}{4}\sin 2\phi\} + M_0 r^2 (\phi - \sin\phi),$$

$$\frac{dU}{dM_0} = F_0 r^2 \{1 - \cos\phi\} + H_0 r^2 (\phi - \sin\phi) + M_0 r\phi.$$

These values enable all questions to be solved concerning a circular rib loaded at detached points only. For example, let it be required to determine the bending moment at a given point of one of the rings of a chain composed of circular links. First, suppose the links not to be strengthened by the addition of a cross bar; then, supposing ϕ measured from the point of contact of two links, $H_0 = 0$, $F_0 = -\frac{W}{2}$, where W is the weight which the chain carries; so that we have only to determine M_0, which is done from the equation

$$\frac{dU}{dM_0} = -\frac{W}{2} r^2 (1 - \cos\phi) + M_0 r\phi = 0,$$

where $\phi = \pi$, because U must be estimated through half the ring up to the point of contact of the other ring in contact with it. The same result must manifestly be obtained if $\phi = \frac{\pi}{2}$; in either case

$$M_0 = \frac{Wr}{\pi}.$$

The stress on any section is therefore given by

$$F = F_0 . \cos\phi + H_0 . \sin\phi = -\frac{W}{2} . \cos\phi,$$

$$H = H_0 . \cos\phi - F_0 . \sin\phi = \frac{W}{2} . \sin\phi,$$

$$M = M_0 + F_0 . r . \sin\phi + H_0 r (1 - \cos\phi),$$

$$= \frac{Wr}{\pi} - \frac{W}{2} r . \sin\phi$$

$$= Wr \frac{2 - \pi . \sin\phi}{2\pi}.$$

If the links had each of them a central bar placed at right angles to the line of action of W, then H_0 would be undetermined, and

the equations for determining M_0 and H_0 would be

$$* \frac{dU}{dM_0}=0; \quad \frac{dU}{dH_0}=0,$$

F_0 being replaced by $-\frac{W}{2}$.

4. I shall next calculate the parts of the coefficients due to a continuous load on a circular rib.

First, for the parts due to the weight of the rib, let w be the weight of the rib per unit of length of the axis, so that the weight of a frustum cut off by planes very near to each other is $w \,.\, ds$; then we have

$$p=w \,.\, \cos\phi; \quad q=w \,.\, \sin\phi,$$

$$K=\left\{1+\frac{d^2}{d\phi^2}\right\}^{-1}\left\{q\rho-\frac{d}{d\phi}\{p\rho\}\right\}$$

$$=wr\left\{1+\frac{d^2}{d\phi^2}\right\}^{-1}\{2 \,.\, \sin\phi\}$$

$$=-wr \,.\, \phi \,.\, \cos\phi,$$

$$M=\int_0^\phi K\rho d\phi=-wr^2\int_0^\phi \phi \,.\, \cos\phi \,.\, d\phi$$

$$=-wr^2\{\phi \,.\, \sin\phi+\cos\phi-1\}.$$

On substitution of this value of M in the general values of the coefficients formerly given, the parts of those coefficients due to the weight of the rib are found to be

$$\left.\begin{aligned}
\frac{dU}{dF_0}&=-\tfrac{1}{8}wr^4\{2\phi(\phi-\sin 2\phi)-3 \,.\, \cos 2\phi+8 \,.\, \cos\phi-5\},\\
\frac{dU}{dH_0}&=-\tfrac{1}{8}wr^4\{2\phi(\cos 2\phi-4 \,.\, \cos\phi-6)+24\sin\phi-3 \,.\, \sin 2\phi\},\\
\frac{dU}{dM_0}&=wr^4\{\phi(\cos\phi+1)-2 \,.\, \sin\phi\}.
\end{aligned}\right\} \quad (A)$$

If the rib be supposed loaded with a vertical load of uniform horizontal intensity w', then

$$p=w' \,.\, \cos^2\phi; \quad q=w' \,.\, \sin\phi \,.\, \cos\phi.$$

And by a similar process we find for the parts of the coefficients

* The consideration of the small amount of work done in compressing the central bar is omitted. The question has been inadvertently treated as if W were a thrust instead of a pull on the chain; to obtain results correct in sign, W must be changed into $-W$ wherever it occurs.

due to such a load,

$$\left.\begin{aligned}\frac{dU}{dF_0} &= -w'r^4\left\{\tfrac{1}{3}-\tfrac{3}{8}\cdot\cos\phi+\tfrac{1}{24}\cos 3\phi\right\},\\ \frac{dU}{dH_0} &= \tfrac{1}{8}w'r^4\left\{\sin\phi-\tfrac{1}{3}\sin 3\phi+\sin 2\phi-2\phi\right\},\\ \frac{dU}{dM_0} &= \tfrac{1}{4}w'r^3\left\{\tfrac{1}{2}\sin 2\phi-\phi\right\}.\end{aligned}\right\}\quad\text{(B)}$$

By means of these two sets of equations, combined with the set given above for the arbitrary constants, all questions concerning circular ribs loaded as described can be solved. It is to be observed that ϕ is supposed measured from the crown of the rib. The values of F, H, M at any point distant ϕ from the crown are

$$F = K + F_0\cdot\cos\phi + H_0\cdot\sin\phi,$$

$$H = pr + \frac{dK}{d\phi} + H_0\cdot\cos\phi - F_0\cdot\sin\phi,$$

$$M = r\int_0^\phi K d\phi + F_0 r\sin\phi + H_0 r(1-\cos\phi) + M_0,$$

where

$$p = w\cdot\cos\phi + w'\cdot\cos^2\phi,$$
$$K = -pr\cdot\phi\cdot\cos\phi - \tfrac{1}{2}w'r\cdot\sin 2\phi.$$

For example, suppose a semicircular rib under a vertical load of uniform horizontal intensity w' but of inconsiderable weight. Also let the rib be hinged at its crown and fixed firmly at its springing; then, since the initial section is at the crown $F_0=0$, $M_0=0$, and H_0 is determined from the equation $\frac{dU}{dH_0}=0$, the value of this coefficient being taken from (B) and the corresponding value in the preceding section. Hence we get, remembering that $\phi=\frac{\pi}{2}$,

$$H_0 r^3\left\{\frac{3\pi}{4}-2\right\}+\tfrac{1}{8}w'r^4\{1+\tfrac{1}{3}-\pi\}=0,$$

whence

$$H_0 = \tfrac{1}{6}w'r\cdot\frac{3\pi-4}{3\pi-8} = \tfrac{2}{5}w'r \text{ approximately.}$$

And therefore

$$F = -\tfrac{1}{2}w'r\sin 2\phi + \tfrac{2}{3}w'r\cdot\sin\phi,$$
$$H = w'r\cdot\cos^2\phi - w'r\cdot\cos 2\phi + \tfrac{2}{3}w'r\cdot\cos\phi,$$
$$M = \tfrac{1}{4}w'r^2(\cos 2\phi - 1) + \tfrac{2}{3}w'r^2(1-\cos\phi),$$

which equations determine the stress on every section. From them it appears that $\frac{2}{3}w'r$ is the horizontal thrust nearly; also that M, which for small values of ϕ is negative, increases to a maximum negative value when $\cos\phi=\frac{2}{3}$, then diminishes to zero and changes sign when $\cos\phi=\frac{1}{3}$, afterwards increasing positively to the springing when its value is $\frac{1}{6}w'r^2$.

5. By aid of the general expressions given in (2), the coefficients may be determined for a rib of any form (such that ρ is a simple function of ϕ) loaded in any manner, though the processes are of great complexity, which, however, is probably unavoidable by any method. In cases in which the thrust is very great, it is moreover necessary to take the work done by it into account, whereby a large number of fresh terms will be introduced into the coefficients, but the process is rather complex than difficult. When the rise of the rib is small compared with its span, and it has a vertical load, it is simpler to use rectangular equations of equilibrium; but it is unnecessary to enter into details, as the process is similar to the one already employed. The results are the same as those Professor Rankine has obtained in his work on Civil Engineering. Care must be taken to include every part of the structure in which work is done; thus if abutments yield, the work done in overcoming their resistance must be estimated and added to U.

In the case of stone and brick arches, not only is work done in the arch ring, but also in the mass of material resting on it. The same law of Least Action, however, governs the distribution of the work; and if it were possible to estimate its whole amount, the problem of the arch might, by application of that law, be completely solved.

In my former article I endeavoured to show that if X, Y, Z be the components of one of the forces acting on a perfectly elastic body, u, v, w the displacements of its point of application produced by the action of the forces on the body, then

$$\frac{d\mathrm{U}}{d\mathrm{X}}=u;\quad \frac{d\mathrm{U}}{d\mathrm{Y}}=v;\quad \frac{d\mathrm{U}}{d\mathrm{Z}}=w.$$

But the reasoning is not so conclusive as the following.

Since

$$2\mathrm{U}=\Sigma\{\mathrm{X}u+\mathrm{Y}v+\mathrm{Z}w\}$$

$$2\,.\,\delta\mathrm{U}=\Sigma\{\mathrm{X}\delta u+\mathrm{Y}\delta.\,v+\mathrm{Z}\,.\,\delta w\}+\Sigma\{u\,.\,\delta\mathrm{X}+v\,.\,\delta\mathrm{Y}+w\,.\,\delta\mathrm{Z}\};$$

but $\Sigma\{\mathrm{X}\delta u+\mathrm{Y}\,.\,\delta v+\mathrm{Z}\,.\,\delta w\}$ is the increment of energy expended, which, by the law of conservation of energy, is equal to

δU, the increment of work done; therefore we have also

$$\delta U = \Sigma \{u\delta X + v\delta Y + w\,.\,\delta Z\},$$

whence the above equations follow.

In particular $\frac{dU}{dF_0}$, $\frac{dU}{dH_0}$, $\frac{dU}{dM_0}$ are the displacements of the initial section from its original position produced by the action of the load on the rib. Thus in the case of the chain of circular links, $\frac{1}{EI} \cdot \frac{dU}{dF_0}$ is the stretching of each link; and when multiplied by the number of links, would give the stretching of the whole chain by the action of the weight W.

March 1865.

NOTE.

This article is intended chiefly to illustrate the application of the principle of least action, many details are therefore omitted which are necessary to complete a practical solution of the problem. Also the simplest cases have been chosen, to avoid, as far as possible, the inherent complexity of these problems.

If a chain hang beneath the rib and carry part of the load by vertical struts, then, the form of the chain being known, the load on each strut is known in terms of H'_0 the unknown horizontal tension of the chain; for otherwise the form of the chain would not be preserved. Supposing the struts indefinitely many in number, the intensity of the load on the chain is $H'_0 \frac{d^2y}{dx^2}$; $\frac{d^2y}{dx^2}$ being got from the equation to the chain (x horizontal).

The intensity of the vertical load on the rib is consequently lessened by the quantity $H'_0 \frac{d^2y}{dx^2}$, and the coefficients $\frac{dU}{dH_0}$, $\frac{dU}{dF_0}$, $\frac{dU}{dM_0}$ must be calculated, together with a new coefficient $\frac{dU}{dH'_0}$, in terms of H'_0 and the other quantities involved. U can now be made a minimum, and the four constants F_0, H_0, M_0, H'_0 determined, whereby the problem will be fully solved. If the chain take the thrust of the rib, then $H_0 = H'_0$, and there are only three coefficients. The general problem of the stiffened suspension bridge is a particular case of this more general problem. The elasticity of the chains can be taken into account by estimating the work done in them in terms of H'_0.

Many other problems of the same general class, that is, where the law of variation of stress is known, and its absolute amount is required, may be advantageously treated by application of the principle of least action; in a future article I hope to consider some cases in which the law of variation is required.

LV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 325.]

February 2, 1865.—Major General Sabine President, in the Chair.

THE following communications were read :—

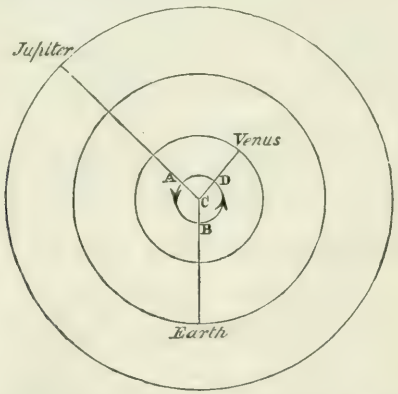
"Researches on Solar Physics.—Second Series. On the Behaviour of Sun-spots with regard to Increase and Diminution." By WarrenDe la Rue, Ph.D., F.R.S., Balfour Stewart, A.M., F.R.S., and Benjamin Loewy, Esq.

One of the authors of this paper having been led, from a preliminary investigation, to suspect that the behaviour of sun-spots with respect to increase and diminution refers to some extraneous influence, they resolved to investigate the behaviour in this respect of the spots observed by Carrington, in addition to the Kew photograms up to the present date.

The authors have thus examined materials embracing a period of ten years, and in this paper state the result.

The nature of their examination is thus described :—

If we imagine great circles of ecliptical longitude to be drawn from the sun's centre, every point of the sun's surface as it moves round by rotation will of course pass successively through each of those great circles, and every one of the planets will do the same as they move round by their own proper motions.



And if we imagine the plane of the paper to denote the plane of the ecliptic, and project upon this plane each body of our system, we shall have a scheme similar to the above, in which ADB, the inner circle, may represent the sun himself, the next circle, let us say the orbit of Venus, the next that of our earth, while the outer may denote the orbit of Jupiter. To an observer looking down upon our system from the north, all motions will be in the direction of the arrow-heads, that is to say, in a direction contrary to that of the hands of a watch, or left-handed, while ecliptical circles of longitude will be represented

by the various radii proceeding from the centre C, the angular difference between the two radii denoting the angular difference between the two correspondin glongitudes. If the observer be stationed at the earth, all points of the solar surface will advance by rotation from left to right across the visible disk; while the radii vectores of the inferior planets Mercury and Venus, which move faster than the earth, will appear to the terrestrial observer to have a left-handed rotation, in such a manner that the planet Venus will move from its place in the diagram to opposition, and ultimately come round to conjunction from the *left*. On the other hand, the superior planets, which move more slowly than the earth, will appear to the terrestrial observer to have a right-handed rotation, in such a manner that Jupiter will proceed from his place in the diagram to opposition, and ultimately come round in conjunction by the *right*. Also the point B, which occupies the central position of the visible solar disk, will have the same heliocentric longitude as the earth. Let us make the central longitude, or longitude corresponding to the position of the earth at the time of observation, our meridian, and let us reckon as negative all longitudes less than 180° to the left, and as positive all those less than 180° to the right. In this way a spot or point of the sun's disk, as it comes round by the left limb, will have the longitude −90, while, as it disappears by the right limb, its longitude will be +90. Hence also the longitude of Jupiter in the diagram will be −ACB, while that of Venus will be +BCD. If the angle ACB is very large, we may say that Jupiter is much to the left, and if BCD is large, we may say that Venus is much to the right. In the examination to which the spots have been subjected, it has been endeavoured to ascertain, as nearly as possible, at what longitude any spot breaks out, or at what longitude it reaches its maximum and begins to wane. Very often, however, we are not able to assign the exact longitude of such an occurrence; but yet, as will be seen in the sequel, we are able to determine, in a general way, the behaviour of spots.

The examination was made in the following manner. Mr. Carrington's original drawings were examined by two observers noting the behaviour of each spot, and the results again compared with Carrington's published maps, which give the behaviour of spots from day to day; ultimately a list was obtained, no spot available for comparison being left out. A similar process was followed with regard to the Kew pictures.

It is to be remarked, that in making the examinations of the Carrington pictures, both observers were ignorant of the planetary configurations; and that although with regard to the Kew pictures one observer knew the corresponding planetary configurations, yet his judgment, being checked by his fellow-observer, could not be biassed by any previous speculative views.

In a Table given, showing the behaviour of sun-spots from the beginning of 1854 to the end of 1864, it is seen that different spots occurring about the same time on the sun's disk behave themselves in the same manner; so that if one spot, after making its appear-

ance, increases until the centre line, another will do the same; or if one spot breaks out on the left or on the right, half the other spots about the same period have a tendency to break out on the same half. Examples of these are referred to in the Tables.

The authors suppose that this peculiarity of behaviour of spots can only be explained by reference to some influence from without. Suppose that such an influence, of a nature unfavourable to spot-production, exists, then, as spots are brought round to it by rotation, they will gradually wane; and, on the other hand, as the surface departs from it, spots will break out. But while there is good evidence for believing in the existence of some such influence, it is a very difficult thing to determine its nature, and one which can only be done very imperfectly with our present knowledge.

The authors attempt to answer the following questions. Is this influence stationary? or, if moveable, can it be traced to any of the planets of our system?

The behaviour of each series of groups is then compared with the positions of the three planets, Mercury, Venus, and Jupiter, at the same date; these planets being imagined to be the most influential; since the first, though small, is very near the sun, the second is both near and tolerably large, while the last, although distant, is of very great mass.

In answer to the first question, Is the influence stationary? it may be remarked that if it be so, the difference of behaviour noticed at different periods must be due to the position of the earth, or point of view at these periods with reference to the stationary influence, and hence in similar months of different years we should have a similar behaviour; but it cannot be found from investigations that there is any connexion between a certain behaviour of sun-spots and a certain period of the year, and hence there is no reason to suppose that the external influence is fixed.

In the next place, does this influence, if moveable, move faster or slower than the earth? If faster, it will proceed from conjunction to opposition, passing over the sun's disk from left to right. If we view it as one unfavourable to the production of spots, then, at first, when it is near conjunction, or a little to the left, the sun's surface to the right, receding from it, will break out into spots; but as the influence moves on to the right, spots will come towards it from their first appearance, and will consequently decrease from the first. But if, on the other hand, the influence move more slowly than the earth, it will move from conjunction to opposition, from right to left; so that a tendency of spots to form on the disk will be followed by a tendency to *increase*, not *decrease*, after making their appearane.

The order of the consecutive phenomena will thus be different in the two cases.

It is shown by a Table that a tendency of spots to break out is followed by a tendency of spots to decrease after making their appearance, and it is thereby concluded that the influence moves faster than the earth. This would seem to point to either Mercury or Venus as the agent in this matter, but the behaviour varies too

slowly to be caused by the former. Venus, therefore, appears to be the influencing agent; and whether the behaviour of spots appears to depend on the position of this planet with reference to the earth, or point of view, the following Table, in which the spot-behaviour is compared with the corresponding position of Venus, will show:—

No. of series.	Behaviour.	Position of Venus.
1.	Increase to centre.	A good deal to left.
2.	Break out.	Conjunction.
3.	Decrease.	To right.
5.	Increase.	Near opposition.
6.	Break out.	Near conjunction.
.....	.....	.....
7.	Increase.	Opposition.
8.	Break out.	Near conjunction.
9.	Uncertain behaviour.	To right.
11.	Increase to centre.	Near opposition.
12.	Increase past centre.	Near opposition (to the left).
13.	Break out.	Near conjunction.
15.	Decrease.	To right.
17.	Increase to centre.	Near opposition.
18.	Increase past centre.	Near opposition (to the left).
20.	Break out.	Near conjunction.
22.	Stationary behaviour.	To right.
24.	Increase to centre.	Near opposition.
25.	Break out.	Near conjunction.
*26.	Uncertain behaviour.	To right.
27.	Increase to centre.	Near opposition.
28.	Increase past centre.	Near opposition (to the left).
29.	Break out.	Near conjunction.
31.	Decrease shortly after appearance.	To the right.
32.	Increase to centre.	Near opposition.

It will be seen from this Table that the behaviour of spots appears to be connected with the position of Venus in such a manner that spots dissolve when that part of the sun's surface in which they exist approaches the neighbourhood of this planet; while, on the other hand, as the sun's disk recedes from this planet, spots begin to break out and reach their maximum on the opposite side.

There are a few cases in which Venus and Jupiter are opposed to one another; the authors do not, however, suppose that these instances are sufficient to prove the fact of an action due to Jupiter, but think it right, in alluding to them, to state at the same time the opposed position of the two planets, since this may furnish a possible explanation of the uncertain behaviour of spots by which these series are characterized.

The results of this paper may be stated briefly as follows:—

* Venus and Jupiter are here opposed to one another.

Observed fact.—Spots appearing about the same time on the sun's disk behave in the same manner as they pass from left to right.

Legitimate deduction.—The behaviour of spots is influenced by something from without, and from the nature of the spot-behaviour the authors conclude that this influence travels faster than the earth ; and finally, they find that the behaviour of spots appears to be determined by the position of Venus in such a manner that a spot wanes as it approaches this planet by rotation, and, on the other hand, breaks out and increases as it recedes from the neighbourhood of the planet, reaching its maximum on the opposite side.

In conclusion, it is not meant in this paper to convey the idea that Venus is the cause of the ten-yearly period of sun-spots, but merely that there is a *varying behaviour* of spots which appears to have reference to the position of this planet, or, putting aside the influencing agent, appears to have reference to certain ecliptical longitudes.

"On the Rapidity of the Passage of *Crystalloid* Substances into the Vascular and Non-Vascular Textures of the Body." By Henry Bence Jones, F.R.S. In a Letter to the Secretary.

Dear Dr. Sharpey,—I am anxious that you should read to the Royal Society a short note containing the results of some observations I lately made on the rapidity of the passage of *crystalloid* substances into the vascular and non-vascular textures of the body.

It occurred to me that it might be possible to trace the passage of substances from the blood into the textures of the body by means of the spectrum-analysis, and with the assistance of Dr. Dupré some very remarkable results have been obtained.

Guinea-pigs have chiefly been used for the experiments. Usually no lithium can be found in any part of their bodies. When half a grain of chloride of lithium was given to a guinea-pig for three successive days, lithium appeared in every tissue of the body. Even in the non-vascular textures, as the cartilages, the cornea, the crystalline lens, lithium would be found.

Two animals of the same size and age were taken; one was given 3 grains of chloride of lithium, and it was killed in eight hours ; another had no lithium ; it was also killed, and when the whole lens was burnt at once, no trace of lithium could be found. In the other, which had taken lithium, a piece of the lens, $\frac{1}{20}$th of a pin's head in size, showed the lithium ; it had penetrated to the centre of the lens.

In another pig the same quantity of chloride of lithium was given, and in four hours even the centre of the lens contained lithium.

Another pig was given the same quantity, and it was killed in two hours and a quarter. The cartilage of the hip showed lithium faintly, but distinctly. The outer portions of the lens showed it slightly ; the inner portions showed no trace.

To a younger pig the same quantity was given, and it was killed in thirty-two minutes. Lithium was found in the cartilage of the hip ; in the aqueous humour ; distinctly in the outer part of the lens and very faintly in the inner part.

In an older and larger pig, to which the same quantity was given, lithium after one hour was found in the hip and knee joints very faintly; in the aqueous humour of the eye very distinctly; but none was found in the lens, not even when half was taken for one trial.

Chloride of rubidium in a three-grain dose was not satisfactorily detected anywhere. When 20 grains had been taken, the blood, liver, and kidney showed this substance; the lens when burnt all at once showed the smallest possible trace; the cartilages and aqueous humour showed none, probably because the delicacy of the spectrum-analysis for rubidium is very much less than that for lithium.

A patient who was suffering from diseased heart took some lithia-water containing 15 grains of citrate of lithia thirty-six hours before her death, and the same quantity six hours before death. The crystalline lens, the blood, and the cartilage of one joint were examined for lithium: in the cartilage it was found very distinctly; in the blood exceedingly faintly; and when the entire lens was taken, the faintest possible indications of lithium were obtained.

Another patient took lithia-water containing 10 grains of carbonate of lithia five hours and a half before death: the lens showed very faint traces of lithium when half the substance was taken for one examination; the cartilage showed lithium very distinctly.

I expect to be able to find lithium in the lens after operation for cataract, and in the umbilical cord after the birth of the fœtus.

I am, yours truly,

H. Bence Jones.

February 9.—Major-General Sabine, President, in the Chair.

The following communication was read:—

"Note on the Atomicity of Aluminium." By Professor A. W. Williamson, F.R.S., President of the Chemical Society.

In the "Preliminary Note on some Aluminium Compounds," by Messrs. Buckton and Odling, published in the last Number of the Society's 'Proceedings*,' some questions of considerable theoretical importance are raised in connexion with the anomalous vapour-densities of aluminium ethyle and aluminium methyle. The authors have discovered that the vapour of aluminium methide ($Al^2 Me^6$) occupies rather more than two volumes (H = 1 vol.) at 163°, when examined by Gay-Lussac's process, under less than atmospheric pressure. The boiling-point of the compound under atmospheric pressure is given at 130°, and the compound accordingly boiled a good deal below 130° at the reduced pressure at which the determination was made. The vapour was therefore considerably superheated when found to occupy a little more than two volumes. When still further superheated up to 220° to 240°, it was found to possess a density equivalent to rather less than four volumes at the normal temperature and pressure.

The aluminium ethyle was found to have a density decidedly in excess of the formula $Al^2 Et^6 = 4$ vols., but far too small for $Al^3 Me^6 = 2$ vols. From their analogy to aluminic chloride, $Al^2 Cl^6 = 2$ vols.,

* See also pp. 313 and 316 of the present volume of this Journal..

the methide and ethide might be expected to have vapour-volumes corresponding to $Al^2 Me^6 = 2$ vols., $Al^2 Et^6 = 2$ vols. The authors seem, however, more inclined to doubt the truth of the general principles which lead us to consider these hexatomic formulæ the correct ones, than to doubt their own interpretation of the observations already made upon the new compounds.

Even if the vapour-volume of aluminic chloride had been unknown to us, there were ample grounds for assigning to aluminium methide a molecular formula $Al^2 Me^6$, and a vapour-density corresponding to $Al^2 Me^6 = 2$ vols. ; for the close analogy of aluminic and ferric salts is perfectly notorious, and the constitution $Fe^2 O^3$ for ferric oxide settles $Al^2 O^3$ as the formula for alumina. With regard, however, to the chlorides of these metals, it might be supposed that the formula $Fe Cl^3$ and $Al Cl^3$ would be the most probable molecular formulæ ; and Dr. Odling, in his useful Tables of Formulæ, published in 1864, expressed an opinion in favour of these formulæ by classing as anomalous Deville's vapour-densities, which correspond to the higher formulæ $Al^2 Cl^6$, $Fe^2 Cl^6$. It is well known that Laurent and Gerhardt, whose penetrating minds raised so many vital questions of chemical philosophy, laid down a preliminary rule that every molecule must contain an even sum of the atoms of chlorine, hydrogen, nitrogen, and metals. According to this rule, the formulæ $Al^2 Cl^6$ and $Fe^2 Cl^6$ would have no greater probability than the formulæ $Fe Cl^3$, $Al Cl^3$; and judging by that rule, Dr. Odling naturally preferred the simpler formulæ.

Since Gerhardt's time chemists have, however, extended to the greater number of metals the arguments which proved oxygen to be biatomic ; and we now know that the alkali-metals, the nitrogen series, silver, gold, and boron, may count with the atoms of chlorine, hydrogen, &c. to make up an even number in each molecule, but that the greater number of metals must not be so counted ; for that in each molecule in which they are contained the sum of the atoms of chlorine, hydrogen, nitrogen, potassium, &c. must be even, just as much as if the atom of the diatomic or tetratomic metal were not in the compound. In a paper "On the Classification of the Elements in relation to their Atomicities," I had occasion to point out that inasmuch as iron and aluminium belong, partly by their own properties, partly by their analogies, to the class of metals which do not join with chlorine, &c. in making up an even number of atoms, the number of those other atoms in each molecule must be even in itself, just as if iron or aluminium were not there ; and that accordingly the formulæ $Fe^2 Cl^6$, $Al^2 Cl^6$ are really quite normal. In like manner I showed that the vapour-density of calomel, $HgCl = 2$ vols., is anomalous, as containing in a molecular volume a single atom of chlorine, although, in accordance with Gerhardt's rule, Dr. Odling had classed it as normal. I certainly understood that my able friend accepted my suggestion in this case at least, for he speedily brought forward theoretical and experimental facts in confirmation of it.

These examples serve to show that it was to be expected that the ethyle and methyle compounds of aluminium would contain an even

number of atoms of ethyle and methyle in each molecule, and that their formulæ would accordingly be $Al^2 Me^6$, $Al^2 Et^6$.

It remains for us to consider how the deviation from our theoretical anticipations in the case of aluminium ethyle and the partial deviation in the case of aluminium methyle ought to be treated.

Fortunately we have the benefit of some experience to guide us in this matter, for a considerable number of other compounds have been found to occupy in the state of vapour nearly double the volume which corresponds to one molecule; but, with very few exceptions, all of them have already been proved to have undergone decomposition, so as to consist of two uncombined molecules. Thus sal-ammoniac is admitted to have the molecular formula NH^4Cl; yet in the state of vapour this quantity occupies the volume of nearly two molecules, viz. four volumes. Has the anomaly led us to doubt the atomic weight of chlorine, nitrogen, or hydrogen, or to doubt any other of the results of our comparison of their compounds? or has it led chemists to diffusion experiments with its vapour, proving it to contain uncombined HCl and NH^3, each occupying its own natural volume? Has it not been proved that at the temperature at which sal-ammoniac vapour was measured, its constituents mix either without evolving heat (that invariable function of chemical action), or, according to another experimentalist, with evolution of far less heat than of the whole quantity of hydrochloric acid and ammonia combined, on coming together at that high temperature?

Again, SO^4H^2 is known to represent the formula of one molecule of hydric sulphate, yet the vapour formed from it occupies nearly the bulk of two molecules. Has this fact cast any doubt on the atomic weights of the elements S, O, or H? Or has it led to the discovery of peculiarities in the constitution of the vapour which would probably have escaped notice had they not been anticipated by theory, peculiarities which go a long way towards bringing the apparent anomalies within the law?

Nitric peroxide, N^2O^4, was considered, from our knowledge of other volatile compounds of nitrogen, to be anomalous in its vapour-volume being $N^2O^4 = 4$ vols.; and we have been shown by the experiment of Messrs. Playfair and Wanklyn, that the anomaly almost disappears when the compound is evaporated by the aid of a permanent gas at a temperature considerably below its boiling-point, as its theoretical molecule N^2O^4 is then found to occupy the two volumes which every undecomposed molecule occupies. This explanation seems to me to be the more entitled to grave consideration on the part of the discoverers of the new aluminium compounds, from the fact that the evidence in favour of it has been admitted to be conclusive by Dr. Odling, who classes nitric peroxide by the formula $N^2O^4 = 2$ vols. among compounds with normal vapour-densities, in virtue of the fact that at low temperatures it can be obtained with that density, though having half that density at higher temperatures.

The arguments for admitting that the low vapour-densities of the aluminium compounds are anomalous are even stronger than those which are admitted in the case of nitric peroxide; for it did require

very severe superheating to get the aluminium compounds to near four volumes, whereas it required very ingenious devices to get nitric peroxide out of the four-volume state.

Such guiding principles as we have acquired in chemistry are the noblest fruits of the accumulated labours of numberless patient experimentalists and thinkers; and when any new or old fact appears to be at variance with those principles, we either add to our knowledge by discovering new facts which remove the apparent inconsistency, or we put the case by for a while and frankly say that we do not understand it.

The decision of the atomic weight of aluminium has involved greater difficulty than was encountered in the case of most other metals, owing to the fact of our knowing only one oxide of the metal, and salts corresponding to it; but the analogies which connect aluminium with other metals are so close and so numerous, that there are probably few metals of which the position in our classification is more satisfactorily settled. We may safely trust that the able investigators who are examining these interesting compounds will bring them more fully than now within the laws which regulate the combining proportions of their constituent elements; for, as it now stands, the anomaly is far less than many others which have been satisfactorily explained by further investigations.

Meanwhile aluminium is a metal singular for only appearing in that pseudo-triatomic character in which iron and chromium appear in their sesquisalts.

GEOLOGICAL SOCIETY.

[Continued from p. 326.]

January 25, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "Notes on the Climate of the Pleistocene epoch of New Zealand." By Julius Haast, Ph.D., F.G.S.

The main feature in this communication was a notice of the occurrence of bones of the *Dinornis* in the moraines of the extinct glaciers of New Zealand. In support of the author's opinion that the extinction of that bird was due to the agency of man at a somewhat recent date, it was observed that the present Alpine flora furnished a large quantity of nutritious food quite capable of sustaining the life even of so large a creature; and as the fruits of these plants were at present applied to no apparent purpose in the economy of nature, the author argued the former existence of an adequate amount of animal life to prevent an excessive development of vegetation. This part, he considered, was played by the *Dinornis*.

2. "On the Order of Succession in the Drift-beds in the Island of Arran." By James Bryce, M.A., LL.D., F.G.S.

In a paper read last year before the Royal Society of Edinburgh, the Rev. R. B. Watson described all these beds as Boulder-clay, and did not assign the Shells which he had discovered in them to any particular part of the deposit. Dr. Bryce dissented from this view, and in this paper pointed out the various causes of error likely

to mislead an observer in examining such accumulations. He then described the various sections of the deposits, and showed that the lowest bed is a hard tough unstratified clay, full of striated, smoothed, and polished stones of all sizes, but totally devoid of fossils, and that it is, in fact, the true old Boulder-clay of the geologists of the West of Scotland. The Shells are entirely confined to a bed of clay of open texture, containing a few small stones; it rests immediately on the Boulder-clay as above defined, and is succeeded by various drift-beds, consisting of seams of clay and sand intermingled, containing stones that are rarely striated, and without Shells.

Dr. Bryce then discussed the probable origin of these drifts, and the amount of depression which the land had sustained before the Shell-bed was deposited over the Boulder-clay, which he considered to have been formed by land-ice emanating from central snow-fields, and covering the whole surface of the country.

3. "On the Occurrence of Beds in the West of Scotland in the position of the English Crag." By James Bryce, M.A., LL.D., F.G.S.

In consequence of the results arrived at from the investigation of the Drift-beds of Arran, Dr. Bryce determined to examine all the recorded cases of fossils occurring in the Boulder-clay, the Chapel Hall case having, however, been already undertaken by the Rev. H. W. Crosskey. The most celebrated case is that of the occurrence of Elephant-remains at Kilmaurs, near Kilmarnock, in Ayrshire; and the author showed, from a section of the quarry exposed for the purpose by Mr. Turner, of Dean Castle, which corresponded exactly with one already furnished to him by an aged quarryman, that the Elephant-remains, the Reindeer's horn, and the Shells, all occurred in beds below the Boulder-clay, and not *in* that deposit, as has always been stated. The same conclusion was arrived at respecting the occurrence of Elephant-remains at Airdrie and Bishop briggs, and of Reindeer's horn with Shells at Croftamie; and the author concluded by discussing the question whether the fossils belong to the Upper Crag period, or merely indicate a downward extension of the Arctic fauna which characterizes the beds directly above the Boulder-clay, as described in the last paper.

4. "On the *Tellina proxima* Bed at Chapel Hall, near Airdrie." By the Rev. H. W. Crosskey.

One of the most perplexing cases in Scotland, upon any theory of the formation of Boulder-clay, has been the alleged occurrence at Chapel Hall of a clay-bed containing *Tellina proxima*, intercalated between two masses of true Boulder-clay. The Shells were first found by Mr. James Russell in sinking a well; and the case was made known by Mr. Smith, of Jordan Hill, in a paper laid before the Geological Society in 1850. At the author's request, Mr. Russell had sunk another well 7 yards from the former, from an examination of which Mr. Crosskey satisfied himself that the bed above that containing the Shells is not the true Boulder-clay, but an upper Drift, and that the Shells occurred in a hollow of the lower clay, or true Till, filled up with a clay-deposit of an age interme-

diate between that of the other two. He therefore considers that this can no longer be regarded as one of fossils occurring in the true Boulder-clay.

February 8, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read :—

1. "On the Sources of the Mammalian fossils of the Red Crag, and on the Discovery of a new Mammal in that Deposit allied to the Walrus." By E. Ray Lankester, Esq.

The Mammalian fossils of the Red Crag were stated to belong to three groups :—(1) the teeth of *Coryphodon*, &c., derived from Lower Eocene strata ; (2) the other terrestrial Mammalia ; and (3) the Cetaceans. The *Molluscan* fauna of the Red Crag was cited in proof of its identity in age with the Upper or Yellow Crag of Antwerp, which contained none of the Red Crag Mammals. The underlying Middle and Black Sands of Antwerp contain far larger percentages of extinct forms and very abundant Cetacean remains. The deposits at Darmstadt and in the South of France, containing terrestrial Mammalia similar to those of the Red Crag, are also anterior to the Yellow Crag of Antwerp. The Red Crag was thus shown to include Mammalian fossils found nowhere else excepting in strata of an earlier age. The probabilities therefore were, that these various Mammalia were not indigenous to the Red Crag, but were derived from the breaking up of earlier strata ; and this supposition was supported by lithological evidence, which the author gave in detail, and discussed the chemical and mineralogical questions involved. Further evidence of the extraneous nature of the Mammalian fossils was also adduced, in the fact that teeth of *Rhinoceros* and *Mastodon* occurred at the base of the Coralline Crag ; and other less conclusive facts were cited. The great abundance and perfect condition of teeth of *Carcharodon* and Ziphioid Cetaceans in the Middle Crag of Antwerp, their absence in the Yellow Crag of that locality, and their presence in a much rolled, indurated and fragmentary condition in the Red Crag, often with portions of their previous sandy matrix adhering, was considered as conclusive evidence with regard to the Cetacean remains.

Mr. Lankester then described the tusks of an animal allied to the Walrus, but probably much larger, which he proposed to call *Trichechodon Huxleyi*. The minute details of form and structure were entered into, and the author stated that the teeth called *Balænodon* by Professor Owen belonged really to two genera, *Zihpius* and *Squalodon*, as shown by the remains from the Middle Antwerp beds.

2. "Note on the Geology of Harrogate." By Professor John Phillips, M.A., F.R.S., F.G.S.

The cuttings on the North-eastern Railway, combined with sections exposed in several quarries, have enabled the author to trace the range of the Millstone-grit, Calcareous roadstone, and Yoredale Shales near Harrogate ; and have also thrown some light on the relation of the Permian to the more ancient rocks. Prof. Phillips was also enabled to refer the mineral springs, with greater confidence than heretofore, to a deep source along an axis of movement ;

and to suggest that the Harrogate roadstone probably corresponds to the Main, or 12 fathom, limestone at the top of the Yoredale series. These results, the arguments and facts in support of them, and the inferences obtainable from their consideration were given by the author in this paper, which was illustrated by a horizontal section from Wharfe, on the S.E., through Harrogate, to Nid on the N.W.

February 22, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Lower Silurian Rocks of the South-East of Cumberland and the North-East of Westmoreland." By Professor R. Harkness, F.R.S., F.G.S.

The district described in this paper consists of a narrow band of country on the western side of the Pennine Chain; it possesses external features which indicate a geological structure different from that of the Pennine escarpment, and from that of the adjacent country on the west, from which it is separated by the Pennine fault. Prof. Harkness described the Lower Silurian rocks occupying this narrow tract in some detail, and showed them to consist of Skiddaw Slates, with interstratified greenstone porphyry and ash, and a band of fossiliferous shale. He also gave, in illustration of the structure of the country, a section from Melmerley Scar to Romanfell, and one from Milburn to Dunfell, together with a geological sketch-map of the narrow Lower Silurian tract in question. In conclusion the author described a fault which brings the Skiddaw Slates against the Coniston Limestone, and another, which cuts through the Lower Silurian rocks of the district, having a course at right angles to the former, and nearly parallel to that of the Great Pennine fault.

2. "Note on the Volcanic Tufa of Latacunga, at the foot of Cotopaxi; and on the Cangáua, or Volcanic Mud, of the Quitenian Andes." By R. Spruce, Esq.

The Volcanic Tufa described in this paper is not only used for building-purposes, but also by the smiths instead of charcoal, as when heated to redness it emits considerable heat, but very little flame. The author then described the large deposits of Volcanic Mud, called Cangáua, which are met with throughout the central valley of the Quitenian Andes. This mud is compact, slightly argillaceous, and more or less saline, and occurs in rock-like masses, yielding very slowly to atmospheric agency, or even to running water.

3. "On the Discovery of Flint Implements in the Drift at Milford Hill, Salisbury." By Dr. H. P. Blackmore.

Since the discovery of Flint Implements in the Higher-level gravel at Fisherton on the west of Salisbury, a large number of very excellent weapons have been obtained from the Drift-gravel of Milford Hill. This deposit is of the same age as the Fisherton beds; but it is situated on the opposite side of the Avon, immediately to the east of Salisbury.

Dr. Blackmore described the materials composing the gravel of Milford Hill, and discussed the nature and power of the forces which had brought them together. He then described the position, thickness, and physical relations of the deposit, stating that the gravel is from 10 to 12 feet thick on the top of the hill, becoming thinner and gradually dying away on the sides. The hill itself is quite isolated, being separated from the surrounding higher land by river-valleys; its highest point is about 100 feet above the present level of the rivers.

In making a cutting on the south-eastern side of the hill, a bed of sand containing four species of land-shells was discovered near the base of the gravel. No other fossils have been found in the deposit, with the exception of a single tooth of a species of *Equus*.

Dr. Blackmore concluded by describing the implements themselves, which nearly all belong to the long-pointed type, thus confirming the opinion of Mr. Evans, that this form is mainly characteristic of the Higher-level gravels.

March 8, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Echinodermata from the South-east coast of Arabia, and from Bagh on the Nerbudda." By P. Martin Duncan, M.B, Sec. G.S.

In this paper Dr. Duncan described eight species of Echinoderms, only one of which was new, from Ras Fartak and Ras Sharwên on the south-east coast of Arabia, and four from Bagh on the Nerbudda. He also mentioned five determinable species of other classes from each locality. Of these fossils, *Hemiaster similis*, D'Orb., and *Pecten quadricostatus*, Sow., were alone common to the two localities; but with the exception of the new Echinoderm, which was named *Cottaldia Carteri* by Dr. Duncan, all the species occur in European Cretaceous rocks. He considered the fossils from the two localities to belong to the same period, and discussed the question of the correlation of the deposits containing them with those of Europe, coming to the conclusion that they were most probably of Cenomanian rather than Neocomian age, and of later date than the Pondicherry series; but he also remarked that it is impossible to determine their exact contemporaneity, the vertical range of many of the species being so great, and the parallelism of the allied European Cretaceous beds not exact.

In conclusion, Dr. Duncan discussed several questions arising out of a comparison of fossils from distant localities, especially the specific identity of similar specimens occurring in different formations, or in distant regions; also the variability of certain species, and the idea of "homotaxis."

2. "On the Fossil contents of the Genista Cave at Windmill Hill, Gibraltar." By George Busk, Esq., F.R.S., F.G.S., and the late Hugh Falconer, M.D., F.R.S., F.G.S.

This was a letter addressed by the authors to His Excellency the Governor of Gibraltar, General Sir W. J. Codrington, K.C.B., &c.,

and containing the results of their examination of the Genista Cave. Referring first to Capt. Brome's report for a description of the general features of that cave, the authors stated that the rock of Gibraltar abounds in both seaboard and inland caverns, the Genista Cave being one of the latter class. It has been traced downwards to a depth of 200 feet; but the external aperture has not yet been discovered; it was stated to be full of the remains of Quadrupeds and Birds, some of the former being now wholly extinct, others extinct in Europe and repelled to distant regions of the African continent (as the *Hyæna brunnea*), while others, again, live now either on the rock or in the adjoining Spanish peninsula. A list of the species to which these remains were referable was then given, and it was inferred that there had been a connexion by land, either circuitous or direct, between Europe and Africa at no very remote period. The authors observed that the wild animals whose remains were discovered lived and died upon the rock during a long series of ages, and they gave a detailed account of the manner in which they considered the bones were introduced into the cave. They also recommended the formation of a local collection of these and other specimens, urged the appointment of a geologist to make a geological survey of the rock, and concluded by expressing their opinion of the value and importance of Capt. Brome's exploration of the Genista cavern.

March 22, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "Notes on the Caves of Gibraltar." By Lieutenant Charles Warren, R.E.

The principal caves at Gibraltar are St. Michael's, Martin's, Glen Rocky, Genista, Asylum Tank, Poco Roco, and three under the Signal Station, on the eastern face of the rock. The author described the salient features of St. Michael's Cave, stating that it is a portion of a transverse cleft through the rock, and was probably open to view at no very remote historical period; and he briefly noticed the cave at Poco Roco, which he considers to be a portion of the fissure which extends from Bell Lane, in the town, to the village of Catalan Bay, the noise of blasting having been heard on more than one occasion through the apparently solid rock. In conclusion Lieut. Warren offered his services in the event of a geological survey of Gibraltar being undertaken.

2. "On the asserted occurrence of Human Bones in the ancient fluviatile deposits of the Nile and the Ganges, with comparative remarks on the Alluvial Formation of the two Valleys." By the late Hugh Falconer, M.D., F.R.S., F.G.S.

In this communication the author brought together the few instances on record of the occurrence of mammalian fossil remains in the Valley of the Nile, and instituted a comparison between the Alluvial deposits of the Nile and those of the upper part of the Valley of the Ganges which had come under his own observation. According to certain statements, fossil human bones have been met

with in both of these subtropical valleys ; and Dr. Falconer remarked that at the present time the consideration of the general inferences to which these cases lead may probably be of some use.

After discussing at some length the cases in which human and other Mammalian bones had been stated to occur in the Valley of the Nile, Dr. Falconer described the general features of the Alluvial deposits of the valleys of the Ganges and Jumna, stating what organic remains had been found in them. In a comparison of the two regions, Dr. Falconer observed that there are striking analogies between the Alluvial deposits occurring along the banks of the Nile on the one hand, and the Ganges and Jumna on the other, the most obvious being the great abundance, in both cases, of argillaceo-calcareous concretions, forming an impure kind of travertine, and in the lowermost beds horizontal deposits of the same material ; but that in its poverty of vertebrate remains the former, so far as it has been explored, is a remarkable contrast to the latter.

Dr. Falconer then reverted to an opinion expressed by Sir Proby Cautley and himself many years ago, namely, that the *Colossochelys Atlas* may have lived down to an early epoch of the human period, and become extinct since ; and he concluded with some general observations on the question of the antiquity of the human race, suggested by more recent discoveries.

April 5, 1865.—Sir R. I. Murchison, Vice-President, in the Chair.

The following communications were read :—

1. "On some Tertiary Deposits in the Colony of Victoria, Australia." By the Rev. J. E. T. Woods, F.L.S., F.G.S.

The author first referred to a former paper on the Australian Tertiary strata, and then described the beds of Muddy Creek, near Hamilton, mentioning the principal fossils occurring therein, especially a species of *Trigonia* ; he also stated that the same formation occurs at Harrow, on the River Glenelg, about sixty miles to the north-east, as well as in Tasmania. In discussing the age of these beds he adopted Professor M'Coy's views, that they are of Lower Miocene date ; but he considered the Mount Gambier limestone to be more recent, probably older Pliocene, and the Murray River deposits as possibly holding an intermediate position ; the latter he therefore considered to represent the Upper and Middle Miocene of Europe. Older than all these are certain strata occurring at Port Phillip and elsewhere, which the author referred to the Upper Eocene period. In conclusion Mr. Woods gave a sketch of the salient features of the Bryozoon-faunæ of the deposits occurring at Hamilton and Mount Gambier, chiefly for the purpose of showing that the latter is much the more modern of the two.

In a note, Dr. Duncan enumerated the species of Corals which had been sent him by Mr. Woods ; but he stated that, although they had a very recent aspect, no exact geological date could safely be assigned to them.

2. "On the Chalk of the Isle of Thanet." By W. Whitaker, Esq., B.A., F.G.S., of the Geological Survey of Great Britain.

In this district a bed of comparatively flintless chalk overlies one

with many flints. The higher division, or *Margate Chalk*, contains but few scattered flint-nodules, and shows well-marked N.W. and S.E. joints. The lower division, or *Broadstairs Chalk*, on the other hand, is less jointed, and has many continuous layers of flint. The beds form a very flat arch, as may be seen along the coast from Kingsgate to Pegwell, between which places the flinty chalk rises up from below that with few flints.

It is remarkable that in this neighbourhood the Thanet beds are conformable to the Chalk, the green-coated nodular flints at the bottom of the former resting on a peculiar bed of tabular flint at the top of the latter.

3. "On the Chalk of Buckinghamshire, and on the Totternhoe Stone." By W. Whitaker, Esq., B.A., F.G.S., &c.

In carrying on the geological survey of Buckinghamshire, the Totternhoe Stone (with its underlying chalky marl), which had been sometimes thought to be the representative of the Upper Greensand, was traced south-westwards into a part where that formation was fairly developed, and was then found to overlie it.

The divisions of the Chalk in Buckinghamshire are, in ascending order,—

(1) Chalk-marl, with stony layers here and there, and at top.
(2) The Totternhoe Stone, generally two layers of rather brownish sandy chalk, hard, with dark grains of small brown nodules.
(3) Marly white chalk, without flints.
(4) Hard-bedded white chalk without flints, forming generally a low ridge at the foot of the great escarpment.
(5) The thick mass of white chalk without flints, or with a very few flints in the uppermost part, and at top.
(6) The "chalk-rock," already described in the Society's Journal, a thin hard bed or beds, with green-coated nodules.
(7) The Chalk with flint, the lowermost part only coming on near the top of the escarpment, the rest bed by bed over the tableland southwards, the angle of dip being rather more than that of the slope of the ground.

4. "On the Chalk of the Isle of Wight." By W. Whitaker, Esq., B.A., F.G.S., &c.

The chief object of this paper was to show that here, as in Oxfordshire, &c., the division between the chalk with flints and chalk without flints is marked by a peculiar bed ("chalk-rock"), hard, of a cream-colour, and with irregular-shaped green-coated nodules, which may be seen in many of the pits on the southern flank of the chalk-ridge, where, however, it is very thin.

The author disagreed with the inference that the chalk was eroded before the deposition of the Tertiary beds, which has been drawn from the irregular junction of the two in the cliff-sections, and thought that the irregularity had been caused rather by the formation of "pipes" after the deposition of the latter, although he did not deny that there was other evidence of denudation of the Chalk before the deposition of the Tertiaries upon it.

LVI. *Intelligence and Miscellaneous Articles.*

ON A NEW THERMO-ELEMENT. BY M. S. MARCUS.

THE author has given the following account of the properties and construction of his new thermo-element :—

1. The electromotive force of one of the new elements is $\frac{1}{25}$th of that of a Bunsen's element, and its resistance is equal to 0·4 of a metre of normal wire.

2. Six such elements can decompose acidulated water.

3. A battery of 125 elements disengaged in a minute 25 cubic centims. detonating gas ; the decomposition took place under unfavourable circumstances, for the internal resistance was far greater than that of the interposed voltameter.

4. A platinum wire half a millim. in thickness introduced into the circuit of the same wire is melted.

5. Thirty elements produce an electro-magnet of 150 pounds lifting-force.

6. The current is produced by heating one of the junctions of the elements, and cooling the second by water of the ordinary temperature.

To construct this battery, it is necessary, on the one hand, to procure two electromotors suitable for a thermo-element, and, on the other, to have such an arrangement of the elements, and of the means for heating and cooling, as will ensure as favourable a result as possible. The former constituted the physical, the latter the constructive part of the problem.

In solving the first part of the problem it was the author's endeavour—

a. To use such thermo-elements as are constructed of metals as far apart as possible in the thermo-electric series, and

b. Such as permit great differences of temperature without using ice,—which is only practicable if the bars possess as high fusing-points as possible.

c. The material of the bars must not be costly, and the bars themselves must be easily constructed.

d. The insulation used for the elements must be able to resist high temperatures, and must possess sufficient solidity and elasticity.

As neither the usual bismuth-antimony couples nor any combination of the other simple metals satisfy these conditions, M. Marcus availed himself of the circumstance that alloys, in the thermo-electric pile, do not stand between the metals of which they consist, and was thereby led to the following alloys, which completely satisfy the above requirements :—

For the positive metal—

10 parts of copper,
6 ,, zinc,
6 ,, nickel.

An addition of one part of cobalt increases the electromotive force.

For the negative metal—

12 parts of antimony,
5 ,, zinc,
1 part of bismuth.

By repeated remelting, the electromotive force of the alloy is increased.

Or he used a combination of Argentane (known as Alpacca, from the Triestinghofer metal-manufactory) with the above negative metal; or an alloy of

65 parts of copper,
31 ,, zinc

as positive metal, and an alloy of

12 parts of antimony,
5 ,, zinc

as negative metal.

The bars are not soldered together, but bound by means of screws.

The positive metal melts at about 1200° C., the negative at about 600° C.

As in this element it is only the heating of the positive metal which influences the development of electricity, the arrangement has been made that only this is heated, while the negative metal receives heat by conduction. By this arrangement it is possible to apply temperatures of even 600°, and consequently to attain greater differences of temperature.

An interesting illustration of the conversion of heat into electricity is the fact, that the water which is used for cooling the second point of contact of the element becomes warm very slowly as long as the circuit is closed, but pretty rapidly if it is open.

The thermo-pile in question was constructed with a view to being used with a gas-flame. The individual elements consist of bars of unequal dimensions; the positive electrical bar is 7″ long, 7‴ broad, and $\frac{1}{2}$‴ thick; the negative electrical bar is 6″ long, 7‴ broad, and 6‴ thick. Thirty-two such elements were screwed together, so that all positive bars were upon one, and all negative on the other side, and thus had the form of a grating. The battery consists of two such gratings, which are screwed together in a roof shape, and are strengthened by an iron bar. As an insulator between the iron bar and the elements, mica was used. Besides this, the elements, where they came into contact with the cooling water, were coated with soluble glass. An earthen vessel filled with water was used for cooling the lower contact sides of the elements. The entire battery has a length of 2 feet, a breadth of 6 inches, and a height of 6 inches.

M. Marcus communicated further, that he had constructed a furnace which was intended for 768 elements. They represent a Bunsen's zinc-carbon battery of thirty elements, and consume per diem 240 pounds of coal.—*Sitzungsbericht der Akademie in Wien*, No. 8, 1865.

ON PRODUCTION OF MAGNETISM BY TURNING. BY C. B. GREISS.

On a visit to the central workshop of the Nassau Railway, I was struck by the perfect winding of the shavings of cast steel, which were just like a thin rope; and on investigating a specimen I found that it had two well-pronounced magnetic poles. Subsequently I obtained from the same place a number of shavings of different materials—cast steel, puddle steel, and soft iron. By means of a not at all delicate magnetic needle I found that they all possessed po-

larity; only one required the application of a finer needle. *They had all therefore, by being turned, obtained permanent magnetism, even those which were of soft iron, and which can neither be permanently magnetized by touch nor by the voltaic current.* One of the turnings, 7 feet in length, when broken formed two complete magnets. When a turning of puddle steel was broken it exhibited two poles at the fracture, but not so strong by magnetic as the original ones. Even a piece half an inch in length had two poles.

No connexion could be perceived between the direction of the winding and the occurrence of the poles, analogous to what prevails in magnets on Ampère's theory. In one north pole the windings were in the direction in which the hand of a watch moves, in another in the opposite direction. I found, however, that the turnings had all a sharp edge on one side, but somewhat jagged on the other. The sharp edge is obviously formed where the chisel attacks the metal, so that it can be easily determined where the winding begins and where it ceases. Taking this into account, I found that *in all cases a south pole was formed where the turning began, and a north pole where it ceased.*

In five or six of the pieces the magnetism was stronger than in others. This phenomenon did not originate in the length of the turning nor in their nature, that is, whether of cast steel, puddle steel, or soft iron. But I observed that all turnings whose spires, looked at from the south pole, were in the opposite direction to the motion of the hands of a watch, had a stronger magnetism than those whose spires, looked at from the south pole, coincided in direction with the motion of the hands of a watch.—Poggendorff's *Annalen*, September 1864.

ON THE ALTERATION OF ELECTROMOTIVE FORCE BY HEAT.

BY F. LINDIG.

The following summary is given by the author of the results of an investigation on this subject.

1. Some of the electrical tensions are dependent on the temperature of the exciters, and change more or less with them. Thus, for instance, copper in sulphate of copper, amalgamated zinc in sulphate of zinc and chloride of zinc, and unamalgamated zinc in solution of chloride of sodium, show an increase of force when warmed; while with the ordinary measuring-instruments this cannot be with certainty shown in the case of sulphuric acid and solution of common salt.

2. The change of force is not always in the same direction: thus, while it decreases with the temperature in the case of copper in sulphate of copper, and amalgamated zinc in chloride of zinc and in sulphate of zinc, with unamalgamated zinc it increases in solution of common salt.

3. The change is not in all cases proportional to the change of temperature between the temperatures $+2°$ and $85°$, as is distinctly seen in the case of zinc in sulphate and chloride of zinc.

4. In accordance with (1) and (2), a Daniell's element is not constant with changing temperature when zinc is surrounded with dilute sulphuric acid or solution of common salt.—Poggendorff's *Annalen*, September 1864.

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[FOURTH SERIES.]

JUNE 1865.

LVII. *On the Retardation of Electrical Signals on Land Lines.* By FLEEMING JENKIN, *Esq.**

[With a Plate.]

IN various papers on the theory of the electric telegraph, Professor W. Thomson has investigated the effect produced on electrical signals by lateral induction, and has deduced the laws by which, when certain constants peculiar to each circuit are known, we can calculate the modifications that will occur in any signal, or series of signals, in consequence of transmission through that circuit. The nature of these modifications, which are most marked in the case of submarine cables, may be described as follows.

1. The abrupt well-defined electrical changes produced at the sending-end of the circuit, by making or breaking contact with the battery, are replaced at the receiving-end by changes more or less gradual. Thus, when a current is sent into a cable by suddenly completing the circuit, no sensible effect whatever is at first observed at the further end (supposed in connexion with the earth). After a little while a weak current arrives, and this gradually increases until, after a certain time more or less long, a maximum is reached, when, if the insulation were perfect, the received current would be equal to that sent.

2. If the signals be sent in such rapid succession that sufficient time does not elapse between each change of contact for the received current to acquire this maximum and again fall to zero, then the total magnitude of the change at the receiving-end will be

* Communicated by the Author, being an extension of a paper read at the Meeting of the British Association at Bath.

less than that produced at the sending-end; so that as the rapidity with which the signals succeed one another is increased, the variations in the received currents diminish, and above a certain speed no sensible change whatever in the electrical currents indicating a signal can be observed, however strong the currents produced near the sending-end of the cable may be.

3. The changes in the received currents corresponding to signals sent are not constant even on a given cable with a given battery, but vary according to the electrical condition of the circuit, produced by the signals immediately preceding; so that a given signal at the sending-end does not produce an invariable and constant effect at the receiving-end, but a variable and inconstant effect, depending on preceding signals. Thus when the ordinary electrical signals are sent through a long submarine cable in quick succession, the received signals are not only retarded, but, if sent too quickly, they are weaker than those that would have been received through an aërial line of equal length, resistance, and insulation, they are also confused and unintelligible, and at a still higher speed are wholly obliterated.

The phenomena, as exhibited in long submarine cables, are so marked, and their commercial importance so great, that they have been the subject of considerable study, although even now the constants required for the rigorous application of Professor W. Thomson's theory are imperfectly known*.

Fig. 2, Plate III. is a reduced copy of the curve given by Professor W. Thomson in the Philosophical Magazine for December 1855, and represents the gradual arrival of an electric current at the further end of a long wire in connexion with the earth, when the near or sending end of the line is connected with the battery for a time sufficiently long to allow the current to reach its maximum. The horizontal ordinates represent times in function of a certain quantity a, and the vertical ordinates the strengths of the current at those times. Thus after a time $6a$, the current reaches about 65 per cent. of its maximum strength.

The quantity $a = \frac{kcl^2}{\pi^2} \log_e\left(\frac{4}{3}\right)$, where k = the resistance of the conductor per unit of length in absolute electrostatic measure, c its absolute electrostatic capacity per unit of length, and l its length. l is of course known; k can be pretty accurately calculated from existing data; c is imperfectly known for sub-

* There are not many long and perfectly insulated cables in the world on which the necessary experiments can be tried. The Malta Alexandria cable, belonging to H.M. Government, was singularly adapted for these experiments when first laid; but no use whatever has been made of it for this or any other scientific purpose.

marine cables, and hitherto has been quite unknown for land lines.

The mathematical expression for the above curve will not be found in that paper. The following series, for which the writer is indebted to Mr. Charles Hockin, allows the curve to be easily constructed.

Let C be the maximum current, and let a have the value given above; then calling x the current at any time t, we have

$$x = \mathrm{C}\left\{1 - 2\left\{\left(\frac{3}{4}\right)^{\frac{t}{a}} - \left(\frac{3}{4}\right)^{\frac{4t}{a}} + \left(\frac{3}{4}\right)^{\frac{9t}{a}} - \left(\frac{3}{4}\right)^{\frac{16t}{a}} + \left(\frac{3}{4}\right)^{\frac{25t}{a}} - \&c.\right\}\right\}.$$

The series for uniform imperfect insulation may also be found interesting.

Let n express the quotient of the number expressing resistance to loss from one unit of length of the conductor, divided by the resistance of the same unit of length to conduction along itself, then for uniformly imperfect insulation (the case of a sound submarine cable) we have the current

$$x = \mathrm{C}\left\{1 - 2\,\frac{\epsilon^{\frac{l}{\sqrt{n}}} - \epsilon^{-\frac{l}{\sqrt{n}}}}{2\,\frac{l}{\sqrt{n}}} \times \left(\frac{3}{4}\right)^{\frac{t}{a}\frac{l^2}{\pi^2 n}}\left\{\frac{1}{1+1\cdot\frac{l^2}{n\pi^2}}\left(\frac{3}{4}\right)^{\frac{t}{a}}\right.\right.$$

$$\left.\left. - \frac{1}{1+\frac{1}{4}\cdot\frac{l^2}{n\pi^2}}\cdot\left(\frac{3}{4}\right)^{\frac{4t}{a}} + \frac{1}{1+\frac{1}{9}\cdot\frac{l^2}{n\pi^2}}\cdot\left(\frac{3}{4}\right)^{\frac{9t}{a}} - \frac{1}{1+\frac{1}{16}\cdot\frac{l^2}{n\pi^2}}\cdot\left(\frac{3}{4}\right)^{\frac{16t}{a}} + \&c.\right\}\right\}.$$

In the curve as thus drawn, the maximum current will, when graphically represented, be equal to $10a$.

The object of the present paper is to show the application of the theory to land or aërial lines, and especially to fix an approximate value for the quantity c. The investigation of this subject has hitherto been neglected, inasmuch as the effect of induction in aërial lines is absolutely insensible when any of the ordinary methods of signalling are employed. The charge induced on a conductor separated from all surrounding conductors by many feet of air is exceedingly small, and the time required to allow the received current to reach a maximum is correspondingly short—so short, indeed, that the speed of transmission appears to depend on the skill of the manipulator and the delicacy of the receiving-instruments alone; but when automatic signalling arrangements are employed capable of transmitting a very greatly increased number of words in a given time, the retardation due to lateral induction becomes of importance even on moderate lengths of aërial lines.

One good example of the kind of instrument alluded to is afforded by Professor Wheatstone's automatic arrangement, with which, on a single circuit, 600 letters per minute can be transmitted, each indicated by about three dots on an average, requiring, when reverse currents are used, that they shall be transmitted at the rate of 3600 per minute, or 60 per second.

Other automatic arrangements, for instance that of the Chevalier Bonelli, differing exceedingly in most points from that above mentioned, have this in common, that the number of distinct currents required per minute is exceedingly great; and when these arrangements are used on long aërial circuits, the phenomena of induction place a limit to the number of messages that can be sent in a given time, precisely as the induction in submarine cables limits the capability of the line with ordinary hand-signalling. It becomes, therefore, of importance to the inventors and introducers of these inventions to know where to expect this limit to the rate of transmission, or to the distance over which messages can be sent at a given rate. This consideration led the writer to examine whether any existing experiments gave the data required; and he found that a paper by M. Guillemin, published in 1860*, afforded, for the particular lines of which he made trial, much of the information wanted. M. Guillemin himself appears to have imperfectly understood the theory of Professor W. Thomson; but it is somewhat difficult to be certain as to his views on this point, owing to a very curious coincidence. Ohm, in his paper "On the Mathematical Law of the Galvanic Circuit," published in 1827†, not only considers the case of the permanent current which exists according to the well-known relation between electromotive force and resistance known *par excellence* as Ohm's Law, but also considers the condition of the current when increasing from nothing to its maximum. Ohm does not suppose that this change can take place instantaneously, and upon purely hypothetical grounds he arrived at an expression of the law according to which the increase must take place. This law was wholly unsupported by any practical experiment, and appears at the time to have attracted little or no attention. It is, however, exactly that which was later discovered by Professor William Thomson in a more complete form, and with a knowledge of experimental results justifying his conclusions. Ohm's expression is

$$\gamma \frac{du}{dt} = \chi \frac{d^2u}{dx^2},$$

where u is the potential at the time t, and at a point at the

* *Annales de Chimie et de Physique*, 3 sér. vol. lx.

† *Vide* Taylor's Scientific Memoirs, vol. ii.

distance x from the origin of the cable, χ is the specific conducting-power of the wire, and γ a purely hypothetical quantity, the existence of which Ohm cannot prove, but which he regards as a specific capacity for electricity analogous to the specific capacity of a body for heat, proportional, therefore, to its length and section. Professor William Thomson's expression is

$$kc\frac{dv}{dt}=\frac{d^2v}{dx^2},$$

where v is the u of Ohm's expression, and k and c have the meanings given above. Thus, if we write $\frac{\gamma}{\chi}=kc$, the two expressions become identical. The difference between Ohm's statement and that of Thomson is simply, that whereas the former assumed a certain quantity for which he can give no value, and of the existence of which he can give no proof, the latter uses a quantity, c, of which he can not only prove the existence, but calculate the amount from independent data. Ohm was wrong in supposing the capacity for electricity analogous to that for heat, and proportional to the section of the wire; and therefore no value applicable to all wires even of one metal could be found by experiment for his constants. M. Guillemin starts with Ohm's expression, uses his phraseology, and apparently still believes γ to be a specific capacity proportional to the section of the line and the material employed. He is well aware of the existence of the statical charge, but does not appear to have understood its connexion with the phenomena he observed.

The results, however, of M. Guillemin's experiments are, as was to be expected, in accordance with the completed theory, and allow the constants required for its application to land lines to be calculated with some accuracy.

The direct measurement of the retardation of a single signal in the manner which has been practised for submarine cables would be exceedingly difficult, if not impossible; for not only is the retardation on a long line so trifling as to be measured by a few hundredths of a second, but during that exceedingly short interval it would be necessary, if accurate information were wanted, to measure the gradual increase of the electrical current from nothing to a maximum. The direct measurement of the statical charge, from which the retardation could be readily calculated, could hardly be made on land lines as usually insulated, owing to the loss of electricity by the posts; the experiment would be further vitiated by the so-called polarization at the moist surfaces of the insulators, tending to send a reverse current to that employed to charge the cable. Some indirect

method of observing the phenomena was therefore wanted, and that employed by M. Guillemin leaves little to be desired.

Figs. 3 and 4 will assist in rendering these experiments intelligible. Fig. 3 represents the series of changes produced in the electric current at the receiving-end of a wire by a succession of uniformly long signals, where each contact used to charge and discharge the line is continued for a sufficiently long time to allow the current in each case to rise to its maximum and fall again to zero; c, $c_{\prime}$, $c_{\prime\prime}$ represent the commencement of the charging contacts, and b, $b_{\prime}$, $b_{\prime\prime}$ the commencement of the discharging contacts.

M. Guillemin's experiments have for their object the definition of the curves $c\,b$ and $b\,c$, each of which is identical with that given in fig. 2; and this object he obtains as follows:—

In fig. 4, A L D represents his circuit, formed by a wire leaving Paris and passing through say Tours and Mans back to Paris by a different route.

E and E_1 represent the earth-plates, two in number, G a galvanometer, B the battery. The receiving-end of the line D was kept constantly in connexion with the earth and with one end of the galvanometer-coil. The end A of the line, by means of a cylinder suitably arranged and revolving at a given uniform speed, could be alternately connected with M and N (that is to say, with the battery and earth) for short periods, variable at will, but generally equal to $\frac{1}{10}$th or $\frac{1}{20}$th of a second. The length of these contacts corresponds to the times indicated by the distance separating c and b in fig. 3. These contacts sent a succession of signals round the line to be received at D, where they produced a series of changes in the electric current, such as are represented in fig. 3. This current, it will be observed, does not pass through the galvanometer; but the end F of the galvanometer-coil is so connected with the revolving cylinder, that a contact at F can be established for a very short time, represented by the short space r in fig. 3 (equal, say, to $\frac{1}{1000}$th of a second), and commencing at any given interval t after the contact with the battery at A has been made. Every time such a short contact at D has been completed, a fraction of the received current passing at the moment is diverted, and passes through the galvanometer, tending to deflect the needle. The galvanometer being sensitive, a small fraction of the whole current deflects it sufficiently for the purpose of observation; this derivation exercises very little influence on the whole resistance of the circuit, and does not sensibly disturb the rate of variation in the current received at D. The constant succession of impulses thus given to the galvanometer-needle, produce a constant deflection indicating the strength of the varying received current at a time t

after the contact with the battery at A has been made. The curve of arrival is perfectly defined by using the time t and the observed strength of current as coordinates (the time τ remaining constant); and quite similarly the curve showing the gradual cessation of the current after the earth-contact has been made at the sending-station, is obtained by making the galvanometer-contact at F at intervals of time t after the connexion is made between A and M. By this simple plan, although the time during which the current varies may not exceed in all a few hundredths of a second, the rate of increase or decrease can be perfectly observed. It is to be regretted that M. Guillemin used a simple detector instead of a measuring-galvanometer, by the deflections of which the relative strengths of the current might have been accurately compared. Data are also wanting as to the resistance of the lines on which the experiments were made, also as to their insulation and some other points of interest. These omissions prevent us from being able to calculate with much accuracy the constant c required for the mathematical theory, and render it uncertain whether slight discrepancies between the theoretical and the observed curves are due to the imperfect method, or to necessary conditions on the lines themselves not taken into account by the theory.

M. Guillemin's results confirm the conclusion that the time required to transmit a signal through a uniform conductor, or, more exactly, to allow the received current to reach a certain proportion of its maximum strength, varies as the square of the length of that conductor. He believes that the electromotive force of the battery does exercise a small influence on the rate of transmission, which is contrary to theory; but the effect observed, if any, is very small, not amounting to 10 per cent. when the battery is doubled, and may perhaps be accounted for by considering the improvement which a powerful battery effects on the insulation by producing polarization.

The effect of the resistance was not tested, nor does M. Guillemin seem to have apprehended the manner in which this element enters into the calculation. No experiment on this point, however, is required, as the truth of the theory in this respect has been abundantly proved.

The comparison between the theoretical and observed curves may be made as follows:—M. Guillemin, with a wire 570 kilometres long and 4 millims. in diameter, obtained the results given in the three first lines of the following Table:—

Table I.

Times, in ten-thousandths of a second	17	50	85	127	146	175	
Deviations, in degrees	0·5	2·5	11·5	18·5	22	24·2	26·5
Percentage of maximum deviation	1·9	9·4	41	70	83	91	
Times expressed in function of a according to theoretical curve	1·6	2·2	4·1	6·5	8·5	10·6	
Value of a obtained by dividing the numbers in the first line by those in the fourth	10·6	23	21	19·5	17	16·5	

The meaning of the figures in the Table will be made clearer by an example, thus: 127 ten-thousandths of a second after the signal had been sent, the received current caused a deflection of 18°·5 in the galvanometer, showing that the current had reached 70 per cent. of its maximum strength. The fourth line expresses the fact that, according to Professor Thomson's theory, this fraction of the maximum strength corresponds to a period of 6·5 a; and the last line gives the corresponding value of a (*i. e.* the quotient of 127 by 6·5).

If M. Guillemin's results were in exact accordance with theory, the value of a would be constant from whatever part of the observed curve it might be calculated. We find, however, that the value is least of all at first, rapidly increases, and again slightly decreases. The small value of a obtained from the shortest period of time is possibly due to the want of proportionality between the galvanometer-deflections and the observed current. Still more probably it is due to the difficulty of making an exact observation of so small a deflection as half a degree. We may therefore reject the first result from our calculations. Between one or two degrees and twenty, there is often a pretty good agreement between the deflections shown by the common detector and the current passing through it. We find, therefore, as was to be expected, that in passing from 10 per cent. to 70 per cent. of the maximum current, the value of a varies little. Between 70 and 90 per cent. the values of a again diminish, showing a more rapid increase than would be due to theory. We may therefore say that the observed curve is somewhat flatter than the calculated one, resembling in this respect the curves obtained by the writer in experiments on a submarine cable, and due possibly to a similar cause, viz. varying insulation caused on the land lines by the polarization of the moist film on the insulators—and possibly to electromagnetic induction producing currents in the neighbouring wires.

Tables II. and III. contain the results of two more sets of observations on a line 550 kilometres long. M. Guillemin's paper contains many other Tables; but those selected appear fair examples.

Table II.

Times, in ten-thousandths of a second	17	33	50	65	103	136	146	162	170
Deviations, in degrees	0	2	12	29	42	43·7	44	45·5	46
Percentage of maximum deviation	0	4·2	25	61	88	92	93	96	97
Times expressed in function of a according to the theoretical curve ...	1	1·8	3·2	5·7	10·0	11·0	11·5	13·0	14·0
Value of a obtained by dividing the numbers in the first line by those in the fourth	17	18·3	15·6	11·4	10·3	12·4	12·7	12	12

In Table II. it may be suspected that the effect of imperfect proportionality between the higher galvanometer-deflections and the current makes the agreement between the theoretical and experimental curves better than it should be. This impression is confirmed by the result of Table III., expressing the results of experiments in which the arrangements differed from those which gave Table II. only in the resistance of the circuit of derivation through the galvanometer—a change which diminished the range of the deflections, but can have produced very little alteration in the true received current.

Table III.

Times, in ten-thousandths of a second	17	33	50	65	103	136	150
Deviations, in degrees	0·5	1	4	10	20	23	24·5
Percentage of maximum deviation	2	4	16	40	80	92	98
Times expressed in function of a according to the theoretical curve ...	1·5	1·8	2·6	4·1	8·0	11	
Value of a obtained by dividing the numbers in the first line by those in the fourth	11·4	18·3	19·2	15·8	12·8	12·3	

This series, like Table I., would give a curve considerably flatter than that due to the simple theory.

The effect of imperfect insulation would not bring the theoretical curves nearer to M. Guillemin's results. Imperfect insulation, instead of flattening, rather sharpens the curve, as may be seen from the following Table, calculated by the series given above

for the length of 350 miles, and with the degree of insulation given by good lines in bad weather.

TABLE IV.

Times after making contact, $= \frac{t}{a}$	2	4	6	10	14
Calculated strength of current with perfect insulation for any strength and length of current, maximum being 10	0·64	3·88	6·46	8·58	9·99
Ditto with imperfect insulation. $n=20{,}000$, $l=350$	0·89	4·85	7·5	9·4	9·99
Ditto with imperfect insulation. $n=40{,}000$, $l=350$	1·06	5·56	8·15	9·64	9·99

It is clear that the curves represented by these ordinates depart still more from those given by the experiments; and we may therefore presume that the experiments were tried in fine weather, when the insulation was much better than the above.

The calculation of c, the electrostatical capacity per metre, may be made as follows.

Taking Table I., we find the mean value of a from the last column, neglecting the first number, to be very nearly nineteen and a half 10-thousandths of a second.

The resistance of one kilometre of iron wire, 4 millims. in diameter, is about 9·27 B. A. units, or say 92,700,000 absolute electromagnetic $\frac{\text{metres}}{\text{seconds}}$. This value is obtained from the value of Messrs Digney's resistance-coils expressing one kilometre of iron wire, and exhibited in the International Exhibition for 1862. The value there given from Professor Weber's determination of the metre-second is 100,800,000, and the value taken in the present paper results from the more accurate determination of the Committee on Electrical Standards. The resistance of one metre of the wire is therefore taken as $9{\cdot}27 \times 10^4$ in electromagnetic units.

To convert this into electrostatic measure, it must be divided by $(310{,}740{,}000)^2$ (*vide* Report of British Association for 1863, p. 159); hence

$$k = \frac{9{\cdot}27 \times 10^4}{9{\cdot}656 \times 10^{16}} \text{ equal nearly to } 9{\cdot}6 \times 10^{-13}.$$

$l=570{,}000$; and if we take a equal to 0·00194 second and substitute these values in the equation given above, we obtain the value of $c = \frac{a\pi^2}{kl^2} \frac{1}{\log \frac{4}{3}} = 0{\cdot}213$. The mean value of a from Table II.

for 550 kilometres, excluding the first observations as before, is 0·00132 second; from Table III. it is 0·00153 second. This last value ought to have coincided with the previous one. The mean of the two is 0·00142 second: substituting this value for a, and 550,000 for l in the above expression, we obtain as the value of c, 0·168.

This number should have agreed with the value 0·213 deduced from Table I. The discrepancy shows that it would be of little use to follow out minutely the results to be obtained from every series observed by M. Guillemin.

We are not informed of the resistance in the batteries or at the receiving-end; the resistance of the lines can only be looked on as the roughest approximation.

The state of insulation of the lines is not known, and the deflections observed are not truly proportional to the currents. With all these sources of error, little dependence can be placed on the accuracy of the result; but M. Guillemin has indicated a method by which correct results might be obtained, and has shown that the phenomena of retardation observed on land lines of the usual construction in France is such as would be produced by an electrostatic capacity of between say 0·15 and 0·25 in electrostatic measure. This result is equivalent to saying that the capacity of a metre length of the wire is equal to that of a sphere between 0·15 and 0·25 metre radius, supposed in space at a distance from all conductors; or the capacity of a foot of the wire is equivalent to the capacity of a sphere between 0·15 and 0·25 foot radius under the same conditions.

This result may be compared with the theoretical capacity of a perfectly insulated wire supposed to be suspended in the air at uniform distances above an infinite flat conducting plane, and approached by no other conductor. The capacity of the wire under those circumstances would be

$$C = \frac{1}{2 \log \frac{4h}{D}},$$

where h = the height of the wire above the plane, and D its diameter (*vide* article by Professor William Thomson headed "Telegraph, Electric," *Encyclopædia Britannica*, eighth edition). Taking $h = 3$ metres and $D = 0{\cdot}004$ metre, the above expression gives $C = 0{\cdot}067$, or less than half the smallest value obtained from experiment. It is clear that no better agreement was to be expected; for the conditions in practice are far from agreeing with the postulates of the theory. The above calculated value will be increased by the approach of the posts to the wires, by the proximity of the other conducting-wires, by the capacity of

the insulators (which will probably vary considerably according to the weather), and possibly by the effects of polarization at the points of support. (The value does not vary rapidly with the variation of height above the ground.) The apparent capacity, measured by the method M. Guillemin adopts, will also be affected by the dynamic induction from wire to wire, the effect of which will be to make the capacity appear larger. Considerable variations may therefore be expected to be observed between different lines, even where a wire of the same diameter is used; and the state of the weather may sensibly affect this capacity.

The number of signals which may be sent on a land line, of the construction used in M. Guillemin's experiments, may be deduced from the Table calculated by Professor Thomson, published by the writer, and now given in an abridged form.

Table V.

Period of dots in function of α, $\alpha = \frac{kcl^2}{\pi^2} . \log 10^{\frac{1}{1-v}}$.	Amplitude in percentage of maximum current.
$3{\cdot}0\,\alpha$	2·97
$4{\cdot}0\,\alpha$	6·31
$5{\cdot}0\,\alpha$	10·42
$6{\cdot}0\,\alpha$	14·85
$7{\cdot}0\,\alpha$	19·67
$8{\cdot}0\,\alpha$	24·42
$9{\cdot}0\,\alpha$	29·11
$10{\cdot}0\,\alpha$	33·68

The first column represents the period of time occupied by two equal contacts at the sending-end of a telegraph wire, the first contact being made with the battery end, and the second with earth. The time is expressed in seconds, and must be calculated from α for any given conductor. The second column represents the total variation in the received current which will be produced when a regular succession of contacts is sent at the speed given in the first column. The maximum variation, *i. e.* that produced by a permanent contact with the battery, is called 100.

The value α may be calculated for a given line, using for k the resistance per metre of the conductor in B. A. measure, divided by $9{\cdot}656 \times 10^9$, and using the values of c given in this paper, *i. e.* from 0·15 to 0·25. As an example, if we take $c = 0{\cdot}20$, then α on a line 500 kilometres long would be 0·00112 second. By inspection of the Table, we find that a period of 10α is required to produce a variation in the received current equal to one-third of the maximum current that would be received when

kept permanently flowing through the wire. It is very doubtful whether in practice a smaller variation than this would be available for practical signalling by any of the automatic plans yet invented for aërial lines. This would give for the duration of the shortest signal 0·0112 second, equivalent to about 90 signals per second, or about 5400 per minute; on the Morse alphabet this would correspond to 360 words per minute. On a line of 1000 kilometres this speed would be reduced to 90 words per minute. Thus Professor Wheatstone's automatic instruments, alluded to in the beginning of this paper, may be expected to transmit at their full speed over a line 500 kilometres (or say 300 miles) long; but their speed would necessarily be reduced on a line of double that length, unless, by augmenting the section of the conductor, the value of k be reduced. The value of c would at the same time be increased by doing this; but, as will appear from the formula given above for the suspended wire, its value will not be materially affected within practical limits. The value of k in British electrostatic measure for one foot of No. 8 iron wire is not far from $7{\cdot}7 \times 10^{-14}$, whereas the value of k in British measure for a foot of the French wire is about 9×10^{-14}. This difference in the value of k makes a difference of about 15 per cent. in the length of the line over which a given speed of signalling could be maintained.

By similar means we can calculate the number of words per minute which could be transmitted by any system over any given line. It is to be hoped that experiments will soon be made which will determine the value of c in such a way as to supersede the rough approximation arrived at in the present paper.

LVIII. *On Rational Approximations to the Circle.*

By W. J. Macquorn Rankine, *C.E., LL.D., F.R.SS.L. & E.**

1. THE common method of approximating to the ratio which the circumference of a circle bears to its diameter by means of a series of inscribed and circumscribed *regular polygons*, though practically convenient, is unsatisfactory as regards the illustration of certain general principles, because of the irrationality of the successive steps of the approximation,—the circumferences of the whole of the regular polygons, with the exception of the inscribed hexagon and circumscribed square, being computed by successive extractions of the square root, and expressed by means of surds.

2. The object of the present paper is to show how, by using *irregular* inscribed and circumscribed polygons, an indefinite

* Communicated by the Author.

series of approximations of any degree of closeness to the circumference of a circle may be obtained, consisting wholly of rational quantities, computed by the operations of common arithmetic only.

3. This process is not to be recommended for practical use, because of the extreme length of the calculations required by it; but it appears to me to be worthy of some attention on account of its great simplicity in principle.

4. The method of this paper is so obvious that I was at first unwilling to believe that it could be new; and I now publish it only because I have not been able to find or to hear of any previous publication of it.

5. The following lemmata, or preliminary propositions, are already well known.

Lemma I. Problem.—To construct an angle whose sine and cosine (and therefore its tangent also) shall be rational fractions, and whose sine and tangent shall not differ by more than a given fraction $\left(\frac{1}{m}\right)$ of the tangent.

It is well known that if a and b be any two whole numbers, a^2+b^2, a^2-b^2, and $2ab$ represent the sides of a *rational right-angled triangle*; and therefore either of the acute angles of that triangle fulfils the conditions of the problem. Thus, let θ be the angle opposite the side $2ab$; then

$$\sin\theta=\frac{2ab}{a^2+b^2};\quad \cos\theta=\frac{a^2-b^2}{a^2+b^2};\quad \tan\theta=\frac{2ab}{a^2-b^2}. \quad . \quad (1)$$

To fulfil the condition that $\tan\theta-\sin\theta\leqq\frac{\tan\theta}{m}$, we must have

$$\frac{2b^2}{a^2+b^2}\leqq\frac{1}{m};$$

and consequently

$$a^2\geqq(2m-1)b^2. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Having, then, in the first place assumed any arbitrary value for b, take for a^2 any integer square which is not less than $(2m-1)b^2$, and find the required sine, cosine, and tangent by the equations (1). Q. E. F.

6. Lemma II. Theorem.—If the sines and cosines (and therefore the tangents) of two or more angles are rational fractions, so also are the sine, cosine, and tangent of any angle formed by adding or subtracting those angles; because these quantities are computed from the sines and cosines of the given angles by addition, subtraction, multiplication, and division. For example, in the case of two angles θ and ϕ,

$$\sin(\theta \pm \phi) = \cos\phi \sin\theta \pm \cos\theta \sin\phi;$$
$$\cos(\theta \pm \phi) = \cos\phi \cos\theta \mp \sin\phi \sin\theta;$$
$$\tan(\theta \pm \phi) = \frac{\tan\theta \pm \tan\phi}{1 \mp \tan\phi \tan\theta}.$$

This theorem includes, as a particular case, the proposition that if the sine and cosine of an angle are rational fractions, so also are the sines, cosines, and tangents of all multiples of that angle.

The known formulæ for the sine and cosine of a multiple angle are here given for convenience of reference.

Let $\cos\theta = c$, $\sin\theta = s$; then

$$\cos n\theta = c^n - n \cdot \frac{n-1}{2} c^{n-2}s^2 + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot \frac{n-3}{4} c^{n-4}s^4 - \&c. \quad (3)$$

$$\sin n\theta = nc^{n-1}s - n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} \cdot c^{n-3}s^3 + \&c. \quad . \quad (4)$$

7. Proposition I. Theorem.—*If all the sides of a polygon inscribed in a circle, save one, are known to be equal to the sides of rational right-angled triangles of which the diameter of the circle is the hypothenuse, then the remaining side of that polygon also is equal to the side of a triangle of the same sort; and consequently the perimeter of the polygon is commensurable with the diameter of the circle.*

For the ratios of the given sides of the polygon to the diameter are the sines of a set of angles whose sines and cosines are rational fractions; and the ratio of the remaining side to the diameter is the sine of the sum of those angles, which, by Lemma II., is a rational fraction also, as well as the cosine of that sum; whence the ratio of the perimeter of the polygon to the diameter of the circle is the sum of a set of rational fractions. Q. E. D.

8. Proposition II. Theorem.—*If tangents be drawn to the circle at the angles of the before-mentioned inscribed polygon, so as to make the corresponding circumscribed polygon, every side of that polygon (and therefore its perimeter) will be commensurable with the diameter of the circle.*

For each of the parts (which may be called *half-sides*) into which each side of the circumscribed polygon is divided by its point of contact with the circle bears a ratio to the diameter expressed by a rational fraction (being half the tangent of one of the before-mentioned angles); and the ratio of the perimeter of the polygon to the diameter of the circle, being the sum of those fractions, is a rational fraction also. Q. E. D.

9. Proposition III. Problem.—*Upon a given circle to construct an inscribed polygon and the corresponding circumscribed polygon, so that the perimeters of both shall be commensurable with the diameter of the circle, and shall not differ from each other by more than a given fraction* $\left(\frac{1}{m}\right)$ *of the perimeter of the circumscribed polygon.*

First solution.—By Lemma I., find the sine, cosine, and tangent of an angle (θ) which shall be rational fractions, and such that the difference between the tangent and sine shall not exceed $\frac{1}{m}$ of the tangent.

Then by the ordinary rules (referred to in Lemma II.) compute successively the sines of the multiples of that angle, $\sin 2\theta$, $\sin 3\theta$, &c., until a sine is arrived at, say $\sin n\theta$, less than $\sin\theta$*.

Then the inscribed and circumscribed polygons will each have $n+1$ sides; n of the sides of the inscribed polygon will be equal to each other, and to the diameter of the circle $\times \sin\theta$, and the remaining side to the diameter $\times \sin n\theta$; and $2n$ of the half-sides of the circumscribed polygon will be equal to each other, and to the diameter of the circle $\times \frac{\tan\theta}{2}$, and the remaining two half-sides will be equal to each other, and to the diameter $\times \frac{\tan(-n\theta)}{2}$.

Thus the ratios of the perimeters of the inscribed and circumscribed polygons to the diameter of the circle will be respectively

$$\frac{\text{inscribed polygon}}{\text{diameter}} = n\sin\theta + \sin n\theta, \quad . \quad . \quad . \quad (5)$$

$$\frac{\text{circumscribed polygon}}{\text{diameter}} = n\tan\theta + \tan(-n\theta)\,; \quad . \quad (6)$$

both of which ratios are rational fractions; and the difference between them is less than $\frac{1}{m}$ of the greater of them. Q. E. F.

Second solution.—Proceed as before until $\sin n\theta$ is found less than $\sin\theta$; then find $\sin(n+1)\theta$, which will also be less than $\sin\theta$. The inscribed and circumscribed polygons will each now have $n+2$ sides; $n+1$ of the sides of the inscribed polygon

* The value of n might be found with the aid of trigonometrical tables, by taking the greatest whole number in the quotient $\frac{\pi}{\theta}$; but this would be a departure from the principle of using none but the elementary processes of arithmetic.

will be equal to each other, and to the diameter $\times \sin\theta$; and $n+1$ of the half-sides of the circumscribed polygon will be equal to each other, and to the diameter $\times \frac{\tan\theta}{2}$; the remaining side of the inscribed polygon and the remaining pair of half-sides of the circumscribed polygon will have for their arithmetical ratios to the diameter respectively $\sin(n+1)\theta$ and $\frac{\tan(-n-1)\theta}{2}$, but will be negative; which means that as the first side and the $(n+1)$th side of the inscribed polygon, and the corresponding parts of the circumscribed polygon, will overlap each other, the closing side of the one and pair of half-sides of the other must run backwards, and must be subtracted in the computation of perimeters.

Thus the ratios of the perimeters to the diameter will now be

$$\frac{\text{inscribed polygon}}{\text{diameter}} = (n+1)\sin\theta - \sin(n+1)\theta, \quad . \quad (7)$$

$$\frac{\text{circumscribed polygon}}{\text{diameter}} = (n+1)\tan\theta - \tan(-n-1)\theta, \quad . \quad (8)$$

both of which ratios are rational fractions, and the difference between them is less than $\frac{1}{m}$ of the greater of them. Q. E. F.

10. Proposition IV. Problem.—*In solving the previous problem, to ensure that the denominator of the ratio of the perimeter of the circumscribed polygon to the diameter shall contain any given factor* (f).

In applying Lemma I., make either $a-b$ or $a+b$ a multiple of the given factor; that is to say, having chosen an arbitrary value of b, make

$$a = kf \pm b, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

and determine the multiplier k by trial, so as to satisfy the condition expressed by equation (2), viz.

$$a^2 = (kf \pm b)^2 \geq (2m-1)b^2. \quad . \quad . \quad . \quad . \quad (10)$$

Then $\cos\theta = \frac{a^2-b^2}{a^2+b^2}$ will contain the given factor f, which will therefore be a factor in the denominator of $\tan\theta$, and of the tangent of every *odd* multiple of θ, as may be proved by considering the results of dividing equation (4) by equation (3). Then, in solving the problem of Proposition III., use the *first* solution if n is odd, and the *second* solution if n is even; and the denominator of the ratio of the perimeter of the circumscribed polygon to the diameter will contain the given factor. Q. E. F.

11. It is easy to see that there are an endless variety of

ways of constructing *rational polygons*, as they may be called, the methods described in Propositions III. and IV. having been chosen merely as the simplest. For example, in a circle whose diameter is 1, let there be an inscribed rational polygon one of whose sides is $\sin\theta$. Then if ϕ be any angle less than θ, and such that $\sin\phi$ and $\cos\phi$ are rational, $\sin\phi$ and $\sin(\theta-\phi)$ will be two of the sides of a new rational inscribed polygon, subtending together the same arc that is subtended by $\sin\theta$ in the original polygon; and to the new inscribed polygon will correspond a new circumscribed polygon, which will be rational also.

Glasgow University, April 1865.

LIX. *On the Cause and Cure of Cataract.* *By* Sir David Brewster, *K.H., F.R.S.**

MY attention was called to the subject of Cataract, in consequence of having, about forty years ago, experienced an incipient attack of that complaint, and studied its progress and cure.

While engaged in a game at chess with Sir James Hall, who was a very slow player, I amused myself in the intervals with looking at the streams of light which radiated from the flame of a candle in certain positions of the eyelids. In one of these observations I was surprised by a new phenomenon, of which I did not immediately see the cause. The flame of the candle was surrounded with lines of light, of an imperfectly triangular form, some parts of which were deeply tinged with the prismatic colours. Upon going home from the chess club, this optical figure was seen more distinctly round the moon, and of course it appeared, with more or less brightness, round every source of light.

Having been engaged in examining the structure of the crystalline lens in animals of all kinds, I soon discovered the cause of the phenomenon which I have described. The laminæ of the crystalline lens had separated near its centre, and the separation had extended considerably towards its margin. The albuminous fluid, the *liquor Morgagni*, which so wonderfully unites into one transparent body, as pure as a drop of water, the mass of toothed fibres which compose the crystalline lens, had not been sufficiently supplied; and if this process of desiccation had continued, the whole laminæ of the lens would have separated, and that state of white opacity have been induced which no attempt has ever been made to remove.

The continuance of this affection of the lens was naturally a

* From the Transactions of the Royal Society of Edinburgh, vol. xxiv. part 1. Communicated by the Author.

subject of much anxiety, and I never entertained the slightest hope of a cure. My medical friends recommended the use of what were then called Eye Pills, but having received no benefit from them, and having learned from experience the sympathy between the eye and the stomach, I used every day, and copiously, the *Pulvis salinus compositus,* and at the end of about eight months, when playing at chess in the same apartment, I had the happiness of seeing the laminæ of the lens suddenly brought into optical contact, and the entire disappearance of the luminous and coloured apparition with which I had been so long haunted.

In speculating on the process by which the crystalline lens is supplied with the necessary quantity of fluid, it occurred to me that it might be derived from the aqueous humour, and that cataract might be produced when there was too little water and too much albumen in the fluid which filled the aqueous chamber.

Upon this hypothesis, incipient cataract might be cured in two ways:—

1st. By discharging a portion of the aqueous humour, in the hope that the fresh secretion, by which the loss is repaired, may contain less albumen, and counteract the desiccation of the lens.

2nd. By injecting distilled water into the aqueous chamber, to supply the quantity of humour discharged from it.

The first of these methods I knew to be practicable and safe, from the fact that a surgeon in the Manchester Infirmary, many years ago, tapped the aqueous chamber of a female patient forty times, in the vain hope of curing a case of conical cornea, which he attributed to an excess of aqueous humour. The frequent repetition of this operation shows how rapidly the humour is secreted, and how reasonable it is to suppose that, in the case of cataract, a healthier secretion might be produced under medical treatment.

Although the second method, of injecting distilled water into the aqueous chamber, presents greater difficulties, yet they do not appear to be insuperable. In 1827, when I happened to be in Dublin, I mentioned this method to the celebrated comparative anatomist, Dr. Macartney, who considered it quite practicable. He mentioned to me that a foreign oculist, whose name I forget, had actually injected distilled water into the eye of a patient with the view of supplying the aqueous humour that was lost during the extraction of the lens.

My attention was recalled to these suggestions for treating incipient cataract, by the results of a series of experiments on the changes which take place in the crystalline lenses of the sheep, the cow, and the horse after death. In these experiments,

which were published in the Philosophical Transactions for 1837, the lenses were placed in a glass trough of distilled water, and exposed to polarized light; and the changes thus produced were indicated by variations in the number and character of the polarized rings, and more palpably by the gradual enlargement of the lens. The distilled water passed through the elastic capsule of the lens. The lens increased in size daily, and at the end of several days the capsule burst, leaving the lens in a disorganized state, the outer laminæ being reduced to an albuminous pulp by the water admitted through the capsule.

These experiments have an obvious importance in reference to the cause and cure of the two kinds of cataract to which the human eye is subject. The aqueous humour is in immediate contact with the capsule of the crystalline lens. When, therefore, the humour contains too little water, the lens has not a sufficient supply of the fluid which keeps its fibres and laminæ in optical contact, and hence the laminæ separate, and the lens becomes opake and hard. When, on the contrary, the aqueous humour contains too much water, the capsule introduces the excess into the lens, and produces the more dangerous affection of soft cataract, in which it is difficult either to depress or extract the lens.

In order to cure the first of these kinds of cataract, we must discharge a portion of the aqueous humour, and either supply its place by injecting distilled water, or leave it to nature to supply a more healthy secretion. In order to cure the second kind, we must supply the place of the discharged humour with a solution of albumen; or, as in the first case, leave to nature the production of a more albuminous secretion.

These views have received a remarkable confirmation from recent experiments on the artificial production and removal of cataract in the eyes of animals. Dr. Kind, a German physiologist, whom I met at Nice in 1857, informed me that he had produced cataract in guinea-pigs by feeding them with much salt, and that the cataract disappeared when there was no salt in their food. More recently, Dr. Mitchell*, an American physician, produced cataract by injecting syrup into the subcuticular sacs of frogs; and Dr. Richardson† did the same by injecting syrup into the aqueous chamber of the recently dead eye of a sheep. In the experiment of Dr. Mitchell, the cataract was removed from the living eye of the frog by surrounding the animal with water; and in that of Dr. Richardson, the cataract was removed from the dead eye of the sheep by replacing the syrup with distilled water.

* American Journal of the Medical Sciences, January 1860.

† Medical Times and Gazette, March 31, 1860.

Neither Dr. Mitchell nor Dr. Richardson seem to have been acquainted with my experiments on the changes in the lens after death, published in 1837, and with the theory of the cause and cure of cataract there referred to, nor with the distinct statement of it published in 1836* and, twenty years later, in 1856†. Dr. Richardson, however, has borne ample testimony to its practicability and safety, when he suggests, almost in my own words, "that it would be worth while, in the earliest stage of cataract in the human subject, to let out the aqueous humour, and to refill the chamber with simple water." And he has borne still stronger testimony to its value by congratulating "Dr. Mitchell in having been the earliest experimentalist to elucidate the synthesis of cataract, and to take the first steps towards a rational interpretation of the disease."

The tendency of the human crystalline lens to indurate or soften, by a defect or excess of water in the aqueous humour, may occur at any period of life, and may arise from the general state of health of the patient; but it is most likely to occur at that age (between 40 and 60, and often much earlier) when the lens experiences that change in its condition which requires the aid of spectacles. This change commences at one part of the margin of the lens, where its density is either increased or diminished. Its action in forming a picture on the retina thus becomes unsymmetrical, and vision is sensibly impaired. But when the change has taken place round the margin of the lens, its symmetrical action is restored, and by the use of spectacles the vision becomes as perfect as it was before the change. If glasses are not used when the change is completed, the eye must either strain itself or use a strong light, to produce distinct vision in reading the small type and the imperfect printing which characterizes the daily press; and by both these processes it will, in a greater or less degree, be injured.

It is a strange delusion, arising either from ignorance or vanity, which induces most people to put off the use of spectacles as long as possible. From the instant they are required, spectacles of different focal lengths ought to be used for the different purposes for which distinct vision is required; and the eyes should never do any work, unless they can do it with perfect distinctness and satisfaction. There is no branch of the healing art where science comes so directly and immediately to the relief of impaired functions as that which relates to vision, and none where science has been so imperfectly applied. When the change in question takes place, the eye requires to be carefully watched, and used with the greatest caution; and if there is any appear-

* Reports of the British Association, 1836, p. 111; and 1837, p. 12.

† North British Review, vol. xx. p. 167 (November 1856).

ance of a separation of the fibres or laminæ, those means should be adopted which, by improving the general health, are most likely to restore the aqueous humour to its usual state. Nothing is more easy than to determine the condition of the crystalline lens; and by the examination of a small luminous object placed at a distance, and the interposition of small apertures, and small opake bodies of a spherical form, we can ascertain the exact point in the lens where the fibres and laminæ have begun to separate, and may observe from day to day whether the disease is gaining ground or disappearing.

[Since the preceding paper was read, I have seen a remarkable work, entitled *Etudes Cliniques sur l'évacuation de l'Humeur Aqueuse dans les Maladies de l'Œil*, par Casimir Spirino (Turin, 1862), pp. 500. M. Spirino had, in the course of little more than a year, operated upon forty-five cases of cataract. In many of these the cataract was perfectly cured, and in others the sight was improved. The first case was that of a lady of eighty-one, who had cataract in both eyes. After thirty-two evacuations of the aqueous humour by the same aperture, and almost always two or three times at the same sitting, both cataracts disappeared, the lady was able to read, without glasses, Nos. 3 and 4 of Jaeger's scale, at the distance of 4 or 5 inches, and even to thread a small needle.]

LX. *Further Application of the Principle of Least Action.* By James H. Cotterill, *B.A., Scholar of St. John's College, Cambridge**.

IN two former articles of this Magazine the principle that the work done, in a system in equilibrium of elasticity, is a minimum, has been applied to cases in which the law of variation of internal stress is supposed to be known, and it is required to find the absolute amount of that stress. I shall now consider some cases in which it is required to find the law of variation, chiefly with a view to verify the statement made in a former article, that the results obtained are identical with those obtained by other methods.

1. A thick hollow cylinder is exposed to fluid pressure, the material being perfectly elastic; it is required to find the law of variation of the principal pressures.

Symmetry shows that the principal pressures must be parallel and perpendicular to a radius of the cylinder. Let p and q be these pressures; then if a concentric cylinder, radius r and thickness δr, be conceived divided into equal parts by a longitudinal

* Communicated by the Author.

plane, $2c\frac{d}{dr}\{pr\}.\delta r$ is the force with which these parts are pressed together; and this force is also equal to $2qc\delta r$, where c is the length of the cylinder, so that

$$q=\frac{d}{dr}\{pr\}=p+r\frac{dp}{dr}.$$

Now the work done is by Clapeyron's theorem proportional to $\int\{(p+q)^2-kpq\}rdr$ (since p and q are *principal* pressures), k being a constant depending on the nature of the material*. We must therefore have

$$\int\Big[\{2(p+q)-kq\}\delta p+\{2(p+q)-kp\}\delta q\Big]rdr=0$$

for all forms of $\delta p\,\delta q$ consistent with

$$\delta q=\delta p+r.\frac{d.\delta p}{dr};$$

whence, by the rules of the calculus of variations,

$$\{2(p+q)-kq-\lambda\}r+\frac{d}{dr}\{\lambda r^2\}=0,$$

$$2(p+q)-kp+\lambda=0,$$

where λ is an unknown function of r. Eliminating λ, we obtain

$$\{4-k\}(p+q)=\frac{d}{dr}\{2r^2(p+q)-kpr^2\};$$

but

$$p+q=2p+r\frac{dp}{dr}=\frac{1}{r}\frac{d}{dr}\{pr^2\};$$

$$\therefore(4-k)\frac{d}{dr}\{pr^2\}=\frac{d}{dr}\left\{2r^3\frac{dp}{dr}+(4-k)pr^2\right\};$$

whence

$$2r^3\frac{dp}{dr}=\text{constant},$$

and

$$p=\frac{c_1}{r^2}+c_2,$$

$$q=-\frac{c_1}{r^2}+c_2,$$

* The work done in a parallelopipedal element will not be altered by a quantity of the same order by supposing its sides inclined at an infinitesimal angle. The third principal pressure is parallel to the axis of the cylinder, and may be supposed zero without at all affecting the result.

two equations which determine the law of variation required. These results agree with those obtained by M. Lamé and Professor Rankine.

2. A thin perfectly elastic spring is bent by the action of forces acting on it; it is required to find its form.

Here the work done in the spring is very approximately $\int \frac{M^2}{2EI}\,ds$, where M is the bending moment at the point considered, E the modulus of elasticity, ds an element of arc of the spring-curve. Consequently $\int M^2 ds$ must be a minimum; but it is shown in works on applied mechanics that $M \propto \frac{1}{\rho}$, where ρ is the radius of curvature of the spring at the point considered; and it follows therefore that $\int \frac{ds}{\rho^2}$ must be a minimum. Adopting this principle, the problem is discussed in Jellet's 'Calculus of Variations,' where it is ascribed to D. Bernoulli.

3. A solid smooth hard cylinder fits into a semicircular recess in an indefinite solid of hard material, lined with a thin layer of soft material; to find the law of variation of the pressure on the cylinder.

Here, if the pressure at an angular distance θ from the bottom be p, the work done will be approximately proportional to $\int p^2 d\theta$, which integral must consequently be a minimum, subject to the condition that $\int_0^{\frac{\pi}{2}} p \cdot \cos\theta \cdot r \cdot d\theta$ shall be equal to half the load on the cylinder. Consequently $\int \{p^2 + 2kp\cos\theta\} d\theta$ must be a minimum, where k is an arbitrary constant. Therefore $p = -k\cos\theta$; but if W be the load,

$$W = 2r\int_0^{\frac{\pi}{2}} p\cos\theta\, d\theta$$

$$= -2rk\int_0^{\frac{\pi}{2}} \cos^2\theta\, d\theta = -\tfrac{1}{2}\pi r k;$$

whence

$$k = -\frac{2W}{\pi r},$$

and

$$p = \frac{2W}{\pi r} \cdot \cos\theta,$$

an equation which gives the law of variation of the pressure, and

shows that its maximum intensity is double of its mean intensity. The conditions necessary for the truth of this solution may perhaps be approximately realized in the case of a shaft and its bearing.

4. In the most general case of the equilibrium of elasticity, U the work done is given by the equation

$$2U = \iiint \left[\frac{1+\frac{\lambda}{\mu}}{3\lambda+2\mu} \{N_1+N_2+N_3\}^2 - \frac{N_1N_2+N_1N_3+N_2N_3-T_1^2-T_2^2-T_3^2}{\mu} \right] dx\,dy\,dz,$$

where N_1, N_2, N_3 are the intensities of the normal pressures on three faces of the element perpendicular to the axes of x, y, z respectively, T_1, T_2, T_3 the intensities of the tangential stress on the element parallel to the planes yz, xz, xy respectively, and $\lambda\mu$ are constants depending on the nature of the material which is supposed homogeneous, and of perfect elasticity. The notation, to facilitate comparison, is the same as that employed by M. Lamé.

This value of U must now be made a minimum, subject to three equations of condition necessary for the equilibrium of the element, namely,

$$\frac{dN_1}{dx} + \frac{dT_3}{dy} + \frac{dT_2}{dz} = 0,$$

$$\frac{dT_3}{dx} + \frac{dN_2}{dy} + \frac{dT_1}{dz} = 0,$$

$$\frac{dT_2}{dx} + \frac{dT_1}{dy} + \frac{dN_3}{dz} = 0.$$

(Lamé, *Leçons sur l'Elasticité*, chap. 7.)

Applying Lagrange's process, we obtain three equations, of which one is

$$2\,\frac{1+\frac{\lambda}{\mu}}{3\lambda+2\mu}\{N_1+N_2+N_3\} - \frac{N_2+N_3}{\mu} = \frac{d\phi}{dx},$$

and three other equations, of which one is

$$\frac{2T_1}{\mu} = \frac{d\phi_2}{dz} + \frac{d\phi_3}{dy},$$

ϕ_1, ϕ_2, ϕ_3 being unknown functions of x, y, z.

Now if with M. Lamé we take

$$\theta = \frac{N_1 + N_2 + N_3}{3\lambda + 2\mu},$$

we have

$$N_2 + N_3 = (3\lambda + 2\mu)\theta - N_1,$$

and the first equation becomes

$$2\left(1 + \frac{\lambda}{\mu}\right)\theta - \frac{3\lambda + 2\mu}{\mu}\theta + \frac{N_1}{\mu} = \frac{d\phi_1}{dx},$$

or

$$N_1 = \lambda\theta + \mu\frac{d\phi_1}{dx}.$$

The two other equations may be reduced in like manner; also from the second set of equations,

$$T_1 = \frac{\mu}{2}\left\{\frac{d\phi_2}{dz} + \frac{d\phi_3}{dy}\right\},$$

with two other symmetrical equations.

Now on comparing these equations with M. Lamé's (p. 65, equations (1)), they are seen to be identical on supposing $\phi_1 = 2u$, $\phi_2 = 2v$, $\phi_3 = 2w$; and thus it appears that the results of the application of the principle of Least Action are identical with those obtained by M. Lamé, which furnishes an independent proof of the principle in the case of perfect elasticity.

Before leaving the subject, I will make some general remarks in conclusion.

Though nothing has been strictly proved, except that the variation in the work done in a perfectly elastic body due to a change in the resisting forces is zero, yet it is sufficiently evident that the work is a minimum. That is, of all values of the work possible on the supposition that the resistances in terms of which the work is expressed may be not merely resisting or (as they have been called) "passive" forces, but applied or "active" forces, the least is that which corresponds to the passive forces. I think that, seen from this point of view, the principle of Least Action is nearly self-evident, even without the confirmatory evidence that the variation in the case of a perfectly elastic body is zero; for the change of a force which is really an effect, into a force which is really a cause, must increase directly or indirectly the total effect.

On the other hand, the energy expended may be said to be a maximum. That is, of all values of the energy expended by the really active forces possible on the supposition that those forces may be active which are really passive, the greatest value *physically possible* corresponds to the passive forces. Thus a weight

placed on a beam descends as low as possible; for if any possible different values of the passive forces would enable that weight to descend, it would certainly descend and produce those values. But there is a restriction implied in the words "physically possible," namely, that the energy expended cannot exceed a certain limit consistently with the physical constitution of the mass of matter; and the function representing the energy expended is not a maximum in the technical sense of the word. It is just the same in the case of the principle of least resistance: each resisting force is indeed the least possible; but its limiting value depends on the constitution of the whole mass, and it is not, properly speaking, a minimum, though the least possible. In my first article I attempted to state the conditions imposed by the physical constitution of the mass; they were framed with the view of avoiding any assumption as to its continuity. They may perhaps seem arbitrary and insufficient; but whatever difficulty there may be in stating these conditions correctly, it is clear that (as was pointed out for the conditions stated) they must be introduced in the act of expressing the work in terms of the forces; and this, which is confirmed by the fact that the function representing the work is susceptible of a minimum value, is the reason why results can in general be obtained by the principle of least action, but only in peculiar cases by the principle of least resistance.

The principle employed in these articles has a manifest analogy to the dynamical principle of least action, so that I have ventured to use the same name and even to call it an extension of that principle, and they may perhaps be cases of a general law. But it is not my intention at present to attempt to develope this analogy; the object of this imperfect sketch has simply been to show that the distribution of the resistance of matter to the action of force is governed by a law of Least Action, and that that law is capable of being applied to all, and may advantageously be applied to some of the cases where it is required to find that distribution. Unless Bernoulli's principle mentioned above be an instance, I am not aware that such a method has been hitherto used, and it therefore may perhaps be new. From want of opportunity, however, I am unacquainted with much that has been done on this subject. I especially regret that I have not seen the new edition of the *Leçons* of Navier, referred to by M. de St. Venant in a paper translated in the Philosophical Magazine for January 1865.

April 30, 1865.

NOTE.

If U, the work done in a body, be conceived to be expressed in

terms of the *displacements* of the points of application of the forces acting on its surface, on the supposition that the passive forces are replaced by any active forces which can be in equilibrium with the applied forces, then, since

$$\delta U = \Sigma\{X\delta u + Y\delta v + Z\delta w\},$$

where u, v, w are the component displacements of the point of application of the force whose components are X, Y, Z, we have

$$\frac{dU}{du} = X, \quad \frac{dU}{dv} = Y, \quad \frac{dU}{dw} = Z,$$

equations which express a property of U analogous to the well-known property of the potential energy of a particle consequent on its vicinity to an attracting body, and to the properties of the accumulation of *vis viva* of a dynamical system.

LXI. *On the Elementary Relations between Electrical Measurements.* *By* Professor J. Clerk Maxwell and Fleeming Jenkin, *Esq.**

Part I.—*Introductory.*

1. *Objects of Treatise.*—The progressive extension of the electric telegraph has made a practical knowledge of electric and magnetic phenomena necessary to a large number of persons who are more or less occupied in the construction and working of the lines, and interesting to many others who are unwilling to be ignorant of the use of the network of wires which surrounds them. The discoveries of Volta and Galvani, of Œrsted, and of Faraday are familiar in the mouths of all who talk of science, while the results of those discoveries are the foundation of branches of industry conducted by many who have perhaps never heard of those illustrious names. Between the student's mere knowledge of the history of discovery and the workman's practical familiarity with particular operations which can only be communicated to others by direct imitation, we are in want of a set of rules, or rather principles, by which the laws remembered in their abstract form can be applied to estimate the forces required to effect any given practical result.

We may be called on to construct electrical apparatus for a particular purpose. In order to know how many cells are required for the battery, and of what size they should be, we require to know the strength of the current required, the electromotive force of the cells, and the resistance of the circuit. If

* Reprinted from the Reports of the British Association for 1863.

we know the results of previous scientific inquiry, and are acquainted with the method of adapting them to the case before us, we may discover the proper arrangement at once. If we are unable to make any estimate of what is required before constructing the apparatus, we may have to encounter numerous failures which might have been avoided if we had known how to make a proper use of existing data.

All exact knowledge is founded on the comparison of one quantity with another. In many experimental researches conducted by single individuals, the absolute values of those quantities are of no importance; but whenever many persons are to act together, it is necessary that they should have a common understanding of the measures to be employed. The object of the present treatise is to assist in attaining this common understanding as to electrical measurements.

2. *Derivation of Units from fundamental Standards.*—Every distinct kind of quantity requires a standard of its own, and these standards might be chosen quite independently of each other, and in many cases have been so chosen; but it is possible to deduce all standards of quantity from the fundamental standards adopted for length, time, and mass; and it is of great scientific and practical importance to deduce them from these standards in a systematic manner. Thus it is easy to understand what a square foot is when we know what a linear foot is, or to find the number of cubic feet in a room from its length, breadth, and height—because the foot, the square foot, and the cubic foot are parts of the same system of units. But the pint, gallon, &c., form another set of measures of volume, which has been formed without reference to the system based on length; and in order to reduce the one set of numbers to the other, we have to multiply by a troublesome fraction, difficult to remember, and therefore a fruitful source of error.

The varieties of weights and measures which formerly prevailed in this country, when different measures were adopted for different kinds of goods, may be taken as an example of the principle of unsystematized standards, while the modern French system, in which everything is derived from the elementary standards, exhibits the simplicity of the systematic arrangement.

In the opinion of the most practical and the most scientific men, a system in which every unit is derived from the primary units with decimal subdivisions is the best whenever it can be introduced. It is easily learnt; it renders calculations of all kinds simpler; it is more readily accepted by the world at large; and it bears the stamp of the authority, not of this or that legislator or man of science, but of nature.

The phenomena by which electricity is known to us are of a mechanical kind, and therefore they must be measured by mechanical units or standards. Our task is to explain how these units may be derived from the elementary ones; in other words, we shall endeavour to show how all electric phenomena may be measured in terms of time, mass, and space only, referring briefly in each case to a practical method of effecting the observation.

3. *Standard Mechanical Units.*—In this country the standard of length is one yard, but a foot is the unit popularly adopted. In France it is the ten millionth part of the distance from the pole to the equator, measured along the earth's surface, according to the calculations of Delambre; and this measure is called a metre, and is equal to 3·280899 feet, or 39·37079 inches.

The standard unit of time in all civilized countries is deduced from the time of rotation of the earth about its axis. The sidereal day, or the true period of rotation of the earth, can be ascertained with great exactness by the ordinary observations of astronomers; and the mean solar day can be deduced from this by our knowledge of the length of the year. The unit of time adopted in all physical researches is one second of mean solar time.

The standard unit of mass is in this country the avoirdupois pound, as we received it from our ancestors. The grain is one 7000th of a pound. In the French system it is the gramme, derived from the unit of length by the use of water at a standard temperature as a standard of density. One cubic centimetre of water is a gramme $=15{\cdot}43235$ grains $={\cdot}00220462$ lb.

A Table showing the relative value of the standard and derived units in the British and metrical systems is given in § 55.

The unit of force adopted in this treatise is that force which will produce a unit of velocity in a free unit mass, by acting on it during a unit of time. This unit of force is equal to the weight of the unit mass divided by g, where g is the accelerating force of gravity

$$\left.\begin{array}{l} =32{\cdot}088\,(1+0{\cdot}005133\sin^2\lambda) \text{ in Brit. units} \\ \text{or } =9{\cdot}78024\,(1+0{\cdot}005133\sin^2\lambda) \text{ in met. units} \end{array}\right\} \text{at the level of the sea,}$$

λ being the latitude of the place of observation. A unit of force still very generally adopted is the weight of the standard mass. The value of the new unit is $\frac{1}{g}$ times the old or gravitation unit.

The unit of work adopted in this treatise is the unit of force, defined as above, acting through the unit of space (*vide* § 55).

4. *Dimensions of Derived Units.*—Every measurement of which

we have to speak involves as factors measurements of time, space, and mass only; but these measurements enter sometimes at one power, and sometimes at another. In passing from one set of fundamental units to another, and for other purposes, it is useful to know at what power each of these fundamental measurements enters into the derived measure.

Thus the value of a force is directly proportional to a length and a mass, but inversely proportional to the square of a time. This is expressed by saying that the *dimensions* of a force are $\frac{LM}{T^2}$; in other words, if we wish to pass from the English to the French system of measurements, the French unit of force will be to the English as $\frac{3{\cdot}28 \times 15{\cdot}43}{1} : 1$, or as 50·6 to 1; because there are 3·28 feet in a metre, and 15·43 grains in a gramme. If the minute were chosen as the unit of time, the unit of force would, in either system, be $\frac{1}{3600}$ of that founded on the second as unit.

A Table of the dimensions of every unit adopted in the present treatise is given in § 55.

Part II.—*The Measurement of Magnetic Phenomena.*

5. *Magnets and Magnetic Poles.*—Certain natural bodies, as the iron ore called loadstone, the earth itself, and pieces of steel after being subjected to certain treatment, are found to possess the following properties, and are called magnets.

If one of these bodies be free to turn in any direction, the presence of another will cause it to set itself in a position which is conveniently described or defined by reference to certain imaginary lines occupying a fixed position in the two bodies, and called their magnetic axes. One object of our magnetic measurements will be to determine the force which one magnet exerts upon another. It is found by experiment that the greatest manifestation of force exerted by one long thin magnet on another occurs very near the ends of the two bars, and that the two ends of any one long thin magnet possess opposite qualities. This peculiarity has caused the name of "poles" to be given to the ends of long magnets; and this conception of a magnet, as having two poles capable of exerting opposite forces joined by a bar exerting no force, is so much the most familiar that we shall not hesitate to employ it, especially as many of the properties of magnets may be correctly expressed in this way; but it must be borne in mind, in speaking of poles, that they do not really exist as points or centres of force at the ends of the

bar, except in the case of long, infinitely thin, uniformly magnetized rods.

If we mark the poles of any two magnets which possess similar qualities, we find that the two marked poles repel each other, that two unmarked poles also repel each other, but that a marked and an unmarked pole attract each other. The pole which is repelled from the northern regions of the earth is called a positive pole; the other end the negative pole. The negative pole is generally marked N by British instrument-makers, and is sometimes called the north pole of the magnet, whereas it is obviously similar to the earth's south pole.

The strength of a pole is necessarily defined as proportional to the force it is capable of exerting on any other pole. Hence the force f exerted between two poles of the strengths m and m_1 must be proportional to the product mm_1. The force, f, is also found to be inversely proportional to the square of the distance, D, separating the poles, and to depend on no other quantity; hence we have, unless an absurd and useless coefficient be introduced,

$$f = \frac{m m_1}{D^2}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

from which equation it follows that the unit pole will be that which at unit distance repels another similar pole with unit force; f will be an attraction or a repulsion according as the poles are of opposite or the same kinds. The dimensions of the unit magnetic pole are $\frac{L^{\frac{3}{2}} M^{\frac{1}{2}}}{T}$.

6. *Magnetic Field.*—It is clear that the presence of a magnet in some way modifies the surrounding space, since any other magnet brought into that space experiences a peculiar force. The neighbourhood of a magnet is, for convenience, called a magnetic field; and for the same reason the effect produced by a magnet is often spoken of as due to the magnetic field instead of to the magnet itself. This mode of expression is the more proper, inasmuch as the same or a similar condition of space may be produced by the passage of electrical currents in the neighbourhood, without the presence of a magnet. Since the peculiarity of the magnetic field consists in the presence of a certain force, we may numerically express the properties of the field by measuring the strength and direction of the force, or, as it may be worded, the intensity of the field and the direction of the lines of force.

This direction at any point is the direction in which the force tends to move a free pole; and the intensity, H, of the field is necessarily defined as proportional to the force, f, with which it

acts on a free pole; but this force, f, is also proportional to the strength, m, of the pole introduced into the field, and it depends on no other quantities; hence

$$f = m\mathrm{H}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

and therefore the field of unit intensity will be that which acts with unit force on the unit pole.

The dimensions of H are $\frac{\mathrm{M}^{\frac{1}{2}}}{\mathrm{L}^{\frac{1}{2}}\mathrm{T}}$.

The lines of force produced by a long thin bar magnet near its poles will radiate from the poles, and the intensity of the field will be equal to the quotient of the strength of the pole divided by the square of the distance from the pole; thus the unit field will be produced at the unit distance from the unit pole. In a uniform magnetic field the lines of force, as may be demonstrated, will be parallel; such a field can only be produced by special combinations of magnets, but a small field at a great distance from any one pole will be sensibly uniform. Thus, in any room unaffected by the neighbourhood of iron or magnets, the magnetic field due to the earth will be sensibly uniform; its direction will be that assumed by the dipping-needle.

7. *Magnetic Moment.*—In reality we can never have a single pole entirely free or disconnected from its opposite pole; and it is time to pass to the consideration of the effect produced on a material bar magnet in a magnetic field. In a uniform field two equal opposite and parallel forces act on its poles, and tend to set it with the line joining those poles in the direction of the force of the field. When the magnet is so placed that the line joining the poles is at right angles to the lines of force in the field, this tendency to turn or "couple," G, is proportional to the intensity of the field, H, the strength of the poles, m, and the distance between them, l; or

$$\mathrm{G} = ml\mathrm{H}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

ml, or the product of the strength of the poles into the length between them, is called the magnetic moment of the magnet; and from equation (3) it follows, that, in a field of unit intensity, the couple actually experienced by any magnet in the above position measures its moment. The dimensions of the unit of magnetic moment are evidently $\frac{\mathrm{L}^{\frac{3}{2}}\mathrm{M}^{\frac{1}{2}}}{\mathrm{T}}$.

8. *Intensity of Magnetization.*—The intensity of magnetization of a magnet may be measured by its magnetic moment divided by its volume.

The dimensions of the unit of magnetization are therefore $\frac{M^{\frac{1}{2}}}{L^{\frac{1}{2}}T}$, the same as in the case of intensity of field.

9. *Coefficient of Magnetic Induction.*—When certain bodies, such as soft iron, &c., are placed in the magnetic field, they become magnetized by "induction"; so that the intensity of magnetization is (except when great) nearly proportional to the intensity of the field.

In diamagnetic bodies, such as bismuth, the direction of magnetization is opposite to that of the field. In paramagnetic bodies, such as iron, nickel, &c., the direction of magnetization is the same as that of the field.

The coefficient of magnetic induction is the ratio of the intensity of magnetization to the intensity of the field, and is therefore a *numerical* quantity, positive for paramagnetic bodies, negative for diamagnetic bodies.

10. *Magnetic Potentials and Equipotential Surfaces.*—If we take a very long magnet, and, keeping one pole well out of the way, move the other pole from one point to another of the magnetic field, we shall find that the forces in the field do work on the pole, or that they act as a resistance to its motion, according as the motion is with or contrary to the force acting on the pole. If the pole moves at right angles to the force, no work is done.

The *magnetic potential* at any point in a magnetic field is measured by the work done by the magnetic forces on a unit pole during its motion from an infinite distance from the magnet producing the field to the point in question, supposing the unit pole to exercise no influence on the magnetic field in question. The idea of potential as a mathematical quantity having different values at different points of space, was brought into form by Laplace*. The name of potential, and the application to a great number of electric and magnetic investigations, were introduced by George Green, in his Essay on Electricity (Nottingham, 1828).

An equipotential surface in a magnetic field is a surface so drawn that the potential of all its points shall be equal. By drawing a series of equipotential surfaces corresponding to potentials 1, 2, 3 . . . . . n, we may map out any magnetic field so as to indicate its properties.

The magnetic force at any point is perpendicular to the equipotential surface at that point, and its intensity is the reciprocal of the distance between one surface and the next at that

* *Mécanique Céleste*, liv. iii.

point. The dimensions of the unit of magnetic potential are $\frac{L^{\frac{1}{2}} M^{\frac{1}{2}}}{T}$.

11. *Lines of Magnetic Force.*—There is another way of exploring the magnetic field, and indicating the direction and magnitude of the force at any point. The conception and application of this method in all its completeness are due to Faraday*. The full importance of this method cannot be recognized till we come to electromagnetic phenomena (§§ 22, 23, & 24).

A line, whose direction at any point always coincides with that of the force acting on the pole of a magnet at that point, is called a line of magnetic force. By drawing a sufficient number of such lines, we may indicate the *direction* of the force in every part of the magnetic field; but by drawing them according to rule, we may indicate the intensity of the force at any point as well as its direction. It has been shown† that if, in any part of their course, the number of lines passing through unit of area is proportional to the intensity there, the same proportion between the number of lines in unit of area and the intensity will hold good in every part of the course of the lines.

All that we have to do, therefore, is to space out the lines in any part of their course, so that the number of lines which start from unit or area is *equal* to the number representing the intensity of the field there. The intensity at any other part of the field will then be measured by the number of lines which pass through unit of area there; each line indicates a constant and equal force.

12. *Relation between Lines of Force and Equipotential Surfaces.*—The lines of force are always perpendicular to the equipotential surfaces; and the number of lines passing through unit of area of an equipotential surface is the reciprocal of the distance between that equipotential surface and the next in order—a statement made above in slightly different language.

In a uniform field the lines of force are straight, parallel, and equidistant; and the equipotential surfaces are planes perpendicular to the lines of force, and equidistant from each other.

If one magnetic pole of strength m be alone in the field, its lines of force are straight lines, radiating from the pole equally in all directions; and their number is $4\pi m$. The equipotential surfaces are a series of spheres, whose centres are at the pole, and whose radii are m, $\frac{1}{2}m$, $\frac{1}{3}m$, $\frac{1}{4}m$, &c. In other magnetic arrangements these lines and surfaces are more complicated;

* Experimental Researches, vol. iii. art. 3122 *et passim*.

† *Vide* Maxwell on Faraday's Lines of Force, Cambridge Phil. Trans. 1857.

but in all cases the calculation is simple, and in many cases the lines and surfaces can be graphically constructed without any calculation.

Part III.—*Measurement of Electric Phenomena by their Electromagnetic Effects.*

13. *Preliminary.*—Before treating of electrical measurements, the exact meaning in which the words "quantity," "current," "electromotive force," and "resistance" are used will be explained. But, in giving these explanations, we shall assume the reader to be acquainted with the meaning of such expressions as conductor, insulator, voltaic battery, &c.

14. *Meaning of the words "Electric Quantity."*—When two light conducting bodies are connected with the same pole of a voltaic battery, while the other pole is connected with the earth, they may be observed to repel one another. The two poles produce equal and similar effects. When the two bodies are connected with opposite poles, they attract one another. Bodies, when in a condition to exert this peculiar force one on the other, are said to be electrified, or charged with electricity. These words are mere names given to a peculiar condition of matter. If a piece of glass and a piece of resin are rubbed together, the glass will be found to be in the same condition as an insulated body connected with the copper pole of the battery, and the resin in the same condition as the body connected with the zinc pole of the battery. The former is said to be positively, and the latter negatively electrified. The propriety of this antithesis will soon appear. The force with which one electrified body acts on another, even at a constant distance, varies with different circumstances. When the force between the two bodies at a constant distance, and separated by air, is observed to increase, it is said to be due to an increase in the quantity of electricity; and the quantity at any spot is defined as proportional to the force with which it acts, through air, on some other constant quantity at a distance. If two bodies, charged each with a given quantity of electricity, are incorporated, the single body thus composed will be charged with the sum of the two quantities. It is this fact which justifies the use of the word "quantity."

Thus the quality in virtue of which a body exerts the peculiar force described is called electricity, and its quantity is measured (*cæteris paribus*) by measuring force.

The quantity thus defined produced on two similar balls similarly circumstanced, but connected with opposite poles of a voltaic battery, is equal, but opposite; so that the sum of these two equal and opposite quantities is zero; hence the conception of positive and negative quantities.

In speaking of a quantity of electricity, we need not conceive it as a separate thing, or entity distinct from ponderable matter, any more than in speaking of sound we conceive it as having a distinct existence. Still it is convenient to speak of the intensity or velocity of sound, to avoid tedious circumlocution; and quite similarly we may speak of electricity, without for a moment imagining that any real electric fluid exists.

The laws according to which the force described varies, as the shape of the conductors, their combinations, and their distances are varied, have been established by Coulomb, Poisson, Green, W. Thomson, and others. These will be found accurately described, independently of all hypothesis, in papers by Professor W. Thomson, published in the Cambridge Mathematical Journal, vol. i. p. 75 (1846), and a series of papers in 1848 and 1849.

15. *Meaning of the words "Electric Current."*—When two balls charged by the opposite poles of a battery, with opposite and equal quantities of electricity, are joined by a conductor, they lose in a very short time their peculiar properties, and assume a neutral condition intermediate between the positive and negative states, exhibiting no electrical symptoms whatever, and hence described as unelectrified, or containing no electricity. But during the first moment of their junction, the conductor is found to possess certain new and peculiar properties: any one part of the conductor exerts a force upon any other part of the conductor; it exerts a force on any magnet in the neighbourhood; and if any part of the conductor be formed by one of those compound bodies called electrolytes, a certain portion of this body will be decomposed. These peculiar effects are said to be due to a current of electricity in the conductor. The positive quantity, or excess, is conceived as flowing into the deficiency caused by the negative quantity; so that the whole combination is reduced to the neutral condition. This neutral condition is similar to that of the earth where the experiment is tried. If the balls are continually recharged by the battery, and discharged or neutralized by the wire, a rapid succession of the so-called currents will be sent; and it is found that the force with which a magnet is deflected by this rapid succession of currents is proportional (*cæteris paribus*) to the quantity of electricity passed through the conductor or neutralized per second; it is also found that the amount of chemical action, measured by the weights of the bodies decomposed, is proportional to the same quantity. The currents just described are intermittent; but a wire or conductor, used simply to join the two poles of a battery, acquires permanently the same properties as when used to discharge the balls as above with great rapidity; and the greater the rapidity with which the balls are discharged, the more perfect the similarity of

the condition of the wire in the two cases. The wire in the latter case is therefore said to convey a permanent current of electricity, the magnitude or strength of which is defined as proportional to the quantity conveyed per second. This definition is expressed by the equation

$$C = \frac{Q}{t}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where C is the current, Q the quantity, and t the time. A permanent current flowing through a wire may be measured by the force which it exerts on a magnet; the actual quantity it conveys may be obtained by comparing this force with the force exerted under otherwise similar conditions, when a known quantity is sent through the same wire by discharges. The strength of a permanent current is found at any one time to be equal in all parts of the conductor. Conductors conveying currents exert a peculiar force one upon another; and during their increase or decrease they produce currents in neighbouring conductors. Similar effects are produced as they approach or recede from neighbouring conductors. The laws according to which currents act upon magnets and upon one another will be found in the writings of Ampère and Weber.

16. *Meaning of the words "Electromotive Force."*—Hitherto we have spoken simply of statical effects; but it is found that a current of electricity, as above defined, cannot exist without effecting work or its equivalent. Thus it either heats the conductor, or raises a weight, or magnetizes soft iron, or effects chemical decomposition; in fine, in some shape it effects work, and this work bears a definite relation to the current. Work done presupposes a force in action. The immediate force producing a current, or, in other words, causing the transfer of a certain quantity of electricity, is called an electromotive force. This force is necessarily assumed as ultimately due to that part of a circuit where a "degradation" or consumption of energy takes place: thus we speak of the electromotive force of the voltaic or thermoelectric couple; but the term is used also independently of the source of power, to express the fact that, however caused, a certain force tending to do work by setting electricity in motion does, under certain circumstances, exist between two points of a conductor or between two separate bodies. But equal quantities of electricity transferred in a given time do not necessarily or usually produce equal amounts of work; and the electromotive force between two points, the proximate cause of the work, is defined as proportional to the amount of work done between those points when a given quantity of electricity is transferred from one point to another. Thus if, with equal currents in two distinct conductors, the work done in the one is

double that done in the second in the same time, the electromotive force in the first case is said to be double that in the second; but if the work done in two circuits is found strictly proportional to the two currents, the electromotive force acting on the two currents is said to be the same. Defined in this way, the electromotive force of a voltaic battery is found to be constant so long as the materials of which it is formed remain in a similar or constant condition. The above definitions, in mathematical language, give $W = EC\dot{t}$, or

$$E = \frac{W}{C\dot{t}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

where E is the electromotive force, and W the work done. Thus the electromotive force producing a current in a conductor is equal to the ratio between the work done in the unit of time and the current effecting the work. This conception of the relations of work, electromotive force, current, and quantity will be aided by the following analogy:—A quantity of electricity may be compared to a quantity or given mass of water; currents of water in pipes in which equal quantities passed each spot in equal times would then correspond to equal currents of electricity; electromotive force would correspond to the head of water producing the current. Thus if, with two pipes conveying equal currents, the head forcing the water through the first was double that forcing it through the second, the work done by the water in flowing through the first pipe would necessarily be twice that done by the water in the second pipe; but if twice as much water passed through the first pipe as passed through the second, the work done by water in the first pipe would again be doubled. This corresponds exactly with the increase of work done by the electrical current when the electromotive force is doubled, and when the quantity is doubled.

Thus, to recapitulate, the quality of a battery or source of electricity, in virtue of which it tends to do work by the transfer of electricity from one point to another, is called its electromotive force, and this force is measured by measuring the work done during the transfer of a given quantity of electricity between those points. The relations between electromotive force and work were first fully explained in a paper by Prof. W. Thomson "On the application of the principle of Mechanical Effect to the Measurement of Electromotive Forces," published in the Philosophical Magazine for December 1851.

17. *Meaning of the words "Electric Resistance."*—It is found by experiment, that even when the electromotive force between two points remains constant, so that the work done by the transfer of a given quantity of electricity remains constant, nevertheless, by modifying the material and form of the conductor, this transfer

may be made to take place in very different times; or, in other words, currents of very different magnitudes are produced, and very different amounts of work are done in the unit of time. The quality of the conductor in virtue of which it prevents the performance of more than a certain amount of work in a given time by a given electromotive force is called its electrical resistance. The resistance of a conductor is therefore inversely proportional to the work done in it when a given electromotive force is maintained between its two ends; and hence, by equation (5), it is inversely proportional to the currents which will then be produced in the respective conductors. But it is found by experiment that the current produced in any case in any one conductor is simply proportional to the electromotive force between its ends; hence the ratio $\frac{E}{C}$ will be a constant quantity, to which the resistance as above defined must be proportional, and may with convenience be made equal; thus

$$R = \frac{E}{C}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

an equation expressing Ohm's law. In order to carry on the parallel with the pipes of water, the resistance overcome by the water must be of such nature that twice the quantity of water will flow through any one pipe when twice the head is applied. This would not be the result of a constant mechanical resistance, but of a resistance which increased in direct proportion to the speed of the current; thus the electrical resistance must not be looked on as analogous to a simple mechanical resistance, but rather to a coefficient by which the speed of the current must be multiplied to obtain the whole mechanical resistance. Thus if the electrical resistance of a conductor be called R, the work W is not equal to CRt, but $C \times CR \times t$, or

$$W = C^2Rt^*, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

where C may be looked on as analogous to a quantity moving at a certain speed, and CR as analogous to the mechanical resistance which it meets with in its progress, and which increases in direct proportion to the quantity conveyed in the unit of time.

18. *Measurement of Electric Currents by their Action on a Magnetic Needle.*—In 1820, Oersted discovered the action of an electric current upon a magnet at a distance; and one method of measurement may be based on this action. Let us suppose the current to be in the circumference of a vertical circle, so that in the upper part it runs from left to right. Then a magnet sus-

* By equation (5) we have $W = CEt$; but by equation (6) $R = \frac{E}{C}$; hence $W = C^2Rt$.—Q.E.D.

pended in the centre of the circle will turn with the end which points to the north away from the observer. This may be taken as the simplest case, as every part of the circuit is at the same distance from the magnet, and tends to turn it the same way. The force is proportional to the moment of the magnet, to the strength of the current as defined by § 15, to its length, and inversely to the square of its distance from the magnet.

Let the moment of the magnet be ml, the strength of the current C, the radius of the circle k, the number of times the current passes round the circle n, the angle between the axis of the magnet and the plane of the circle θ, and the moment tending to turn the magnet G, then

$$G = mlC \cdot 2\pi nk \frac{1}{k^2} \cos\theta, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

which will be unity if ml, C, k, and the length of the circuit be unity, and if $\theta = 0°$.

The unit of current founded on this relation, and called the electromagnetic unit, is therefore that current of which the unit of length placed along the circumference of a circle of unit radius produces a unit of magnetic force at the centre.

The usual way of measuring C, the strength of a current, is by making it describe a circle about a magnet, the plane of the circle being vertical and magnetic north and south. Thus, if H be the intensity of the horizontal component of terrestrial magnetism, and G the moment of this on the magnet, $G = mlH \sin\theta$, whence the strength of the current

$$C = \frac{k^2}{2\pi n} H \tan\theta, \quad . \quad . \quad . \quad . \quad . \quad (9)$$

where k is the radius of the circle, n the number of turns, H the intensity of the horizontal part of the earth's magnetic force as determined by the usual method, and θ the angle of deviation of the magnet suspended in the centre of the circle. As the strength of the current is proportional to the tangent of the angle θ, an instrument constructed on this plan is called a tangent galvanometer. The instrument called a sine galvanometer may also be used, provided the coil is circular. The equation is similar to that just given, substituting $\sin\theta$ for $\tan\theta$.

To find the dimensions of C, we must consider that what we observe is the force acting between a magnetic pole, m, and a current of given length, L, at a given distance, L_1, and that this force $= \frac{mCL}{L_1^2}$. Hence the dimensions of C, an electric current thus measured, are $\frac{L^{\frac{1}{2}} M^{\frac{1}{2}}}{T}$.

19. *Measurement of Electric Currents by their mutual action*

on one another.—Hitherto we have spoken of the measurement of currents as dependent on their action upon magnets; but this measurement in the same units can as simply be founded on their mutual action upon one another. Ampère has investigated the laws of mechanical action between conductors carrying currents. He has shown that the action of a small closed circuit at a distance is the same as that of a small magnet, provided the axis of the magnet be placed normal to the plane of the circuit, and the moment of the magnet be equal to the product of the current into the area of the circuit which it traverses.

Thus, let two small circuits having areas A and A_1 be placed at a great distance D from each other in such a way that their planes are at right angles to each other, and that the line D is in the intersection of the planes. Now let currents C and C_1 circulate in these conductors; a force will act between them tending to make their planes parallel, and the direction of the currents opposite. The moment of this couple will be

$$G = \frac{AC \times A_1C_1}{D^3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Hence the unit electric current conducted round two circuits of unit area in vertical planes at right angles to each other, one circuit being at a great distance, D, vertically above the other, will cause a couple to act between the circuits of a magnitude $\frac{1}{D^3}$. The definition of the unit current (identical with the unit founded on the relations given in § 18) might be founded on this action quite independently of the idea of magnetism.

20. *Weber's Electro-Dynamometer*.—The measurement described in the last paragraph is only accurate when D is very great, and therefore the moment to be measured very small. Hence it is better to make the experimental measurements in another form. For this purpose, let a length (l) of wire be made into a circular coil of radius k; let a length (l_1) of wire be made into a coil of very much smaller radius, k_1. Let the second coil be hung in the centre of the first, the planes being vertical and at the angle θ. Then, if a current C traverses both coils, the moment of the force tending to bring them parallel will be

$$G = \tfrac{1}{2}C^2 \frac{ll_1k_1}{k^2} \sin\theta. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

This force may be measured in mechanical units by the angle through which it turns the suspended coil, the forces called into play by the mechanical arrangements of suspension being known from the construction of the instrument. Weber used a bifilar suspension, by which the weight of the smaller coil

was used to resist the moment produced by the action of the currents.

21. *Comparison of the Electromagnetic and Electrochemical action of Currents.*—Currents of electricity, when passed through certain compound substances, decompose them; and it is found that, with any given substance, the weight of the body decomposed in a given time is proportional to the strength of the current as already defined with reference to its electromagnetic effect. The voltameter is an apparatus of this kind, in which water is the substance decomposed. Special precautions have to be taken, in carrying this method of measurement into effect, to prevent variations in the resistance of the circuit, and consequently in the strength of the current. This subject is more fully treated in Part V. §§ 53, 54.

22. *Magnetic Field near a Current.*—Since a current exerts a force on the pole of a magnet in its neighbourhood, it may be said to produce a magnetic field (§ 6), and, by exploring this field with a magnet, we may draw lines of force and equipotential surfaces of the same nature as those already described for magnetic fields caused by the presence of magnets.

When the current is a straight line of indefinite length, like a telegraph-wire, a magnetic pole in its neighbourhood is urged by a force tending to turn it round the wire, so that this force is at any point perpendicular to the plane passing through this point and the axis of the current.

The equipotential surfaces are therefore a series of planes passing through the axis of the current, and inclined at equal angles to each other. The number of these planes is $4\pi C$, where C is the strength of the current.

The lines of magnetic force are circles, having their centres in the axis of the current, and their planes perpendicular to it. The intensity of the magnetic force at a distance, k, from the current is the reciprocal of the distance between two equipotential surfaces, which shows the forces to be $\frac{2C}{k}$.

The work done on a unit magnetic pole in going completely round the current is $4\pi C$, whatever the path which the pole describes.

23. *Mechanical Action of a Magnetic Field on a closed Conductor conveying a Current.*—When there is mechanical action between a conductor carrying a current and a magnet, the force acting on the conductor must be equal and opposite to that acting on the magnet. Every part of the conductor is therefore acted on by a force perpendicular to the plane passing through its own direction and the lines of magnetic force due to the magnet, and equal to the product of the length of the conductor

into the strength of the current, the intensity of the magnetic field, and the sine of the angle between the lines of force and the direction of the current. This may be more concisely expressed by saying that if a conductor carrying a current is moved in a magnetic field, the work done on the conductor by the electromagnetic forces is equal to the product of the strength of the current into the *number* of lines of force which it cuts during its motion.

Hence we arrive at the following general law, for determining the mechanical action on a closed conductor carrying a current and placed in a magnetic field:—

Draw the lines of magnetic force. Count the number which pass through the circuit of the conductor, then any motion which increases this number will be aided by the electromagnetic forces, so that the work done during the motion will be the product of the strength of the current and the number of additional lines of force.

For instance, let the lines of force be due to a single magnetic pole of strength m. These are $4\pi m$ in number, and are in this case straight lines radiating equally in all directions from the pole. Describe a sphere about the pole, and project the circuit on its surface by lines drawn to the pole. The surface of the area so described on the sphere will measure the solid angle subtended by the circuit at the pole. Let this solid angle $=\omega$, then the number of lines passing through the closed surface will be $m\omega$; and if C be the strength of the current, the amount of work done by bringing the magnet and circuit from an infinite distance to their present position will be $Cm\omega$. This shows that the magnetic potential of a closed circuit carrying a unit current with respect to a unit magnetic pole placed at any point is equal to the solid angle which the circuit subtends at that point.

By considering at what points the circuit subtends equal solid angles, we may form an idea of the surfaces of equal potential. They form a series of sheets, all intersecting each other in the circuit itself, which forms the boundary of every sheet. The number of sheets is $4\pi C$, where C is the strength of the current. The lines of magnetic force intersect these surfaces at right angles, and therefore form a system of rings, encircling every point of the circuit. When we have studied the general form of the lines of force, we can form some idea of the electromagnetic action of that current, after which the difficulties of numerical calculation arise entirely from the imperfection of our mathematical skill.

24. *General Law of the Mechanical Action between Electric Currents and other Electric Currents or Magnets.*—Draw the

lines of magnetic force due to all the currents, magnets, &c., in the field, supposing the strength of each current or magnet to be reduced from its actual value to unity. Call the number of lines of force due to a circuit or magnet, which pass through another circuit, the potential coefficient between the one and the other. This number is to be reckoned positive when the lines of force pass through the circuit in the same direction as those due to a current in that circuit, and negative when they pass in the opposite direction.

If we now ascertain the change of the potential coefficient due to any displacement, this increment multiplied by the product of the strengths of the currents or magnets will be the amount of work done by the mutual action of these two bodies during the displacement. The determination of the actual value of the potential coefficient of two things, in various cases, is an important part of mathematics as applied to electricity. (See the mathematical discussion of the experiment, Appendix D. Brit. Assoc. Reports, 1863, p. 168.)

25. *Electromagnetic Measurement of Electric Quantity.*—A conducting body insulated at all points from the neighbouring conductors may in various ways be electrified, or made to hold a quantity of electricity. This quantity (§ 14) is perfectly definite in any given circumstances; it cannot be augmented or diminished so long as the conductor is insulated, and is called the charge of the conductor. Its magnitude depends on the dimensions and shape and position of the insulated and the neighbouring conductors, on the insulating material, and finally on the electromotive force between the insulated and the neighbouring conductors, at the moment when the charge was produced. The well-known Leyden jar is an arrangement by which a considerable charge can be obtained on a small conductor with moderate electromotive force between the inner and outer coatings, which constitute respectively the "insulated" and "neighbouring" conductors referred to in general. We need not enter into the general laws determining the charge, since our object is only to show how it may be measured when already existing; but it may be well to state that the quantity on the charged insulated conductor necessarily implies an equal and opposite quantity on the surrounding or neighbouring conductors.

We have already defined the magnitude of a current of electricity as simply proportional to the quantity of electricity conveyed in a given time, and we have shown a method of measuring consonant with this definition. The unit quantity will therefore be that conveyed by the unit current as above defined in the unit of time. Thus, if a unit current is allowed to flow for a unit of time in any wire connecting the two coatings of a

Leyden phial, the quantity which one coating loses, or which the other gains, is the electromagnetic unit quantity*. The measurement thus defined of the quantity in a given statical charge can be made by observing the swing of a galvanometer-needle produced by allowing the charge to pass through the coil of the galvanometer in a time extremely short compared with that occupied by an oscillation of the needle.

Let Q be the whole quantity of electricity in an instantaneous current, then

$$Q = 2\frac{C_1 t}{\pi}\sin\tfrac{1}{2}i, \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

where C_1 = the strength of a current giving a unit deflection (45° on a tangent or 90° on a sine galvanometer), t = half the period or time of a complete oscillation of the needle of the galvanometer under the influence of terrestrial magnetism alone, and i = the angle to which the needle is observed to swing from a position of rest, when the discharge takes place. C_1 is a constant which need only be determined once for each instrument, provided the horizontal force of the earth's magnetism remain unchanged. In the case of the tangent galvanometer, the formula for obtaining it has already been given. From equations (9) and (12) we have for a tangent galvanometer

$$Q = \frac{k}{\pi^2 n} H t \sin\tfrac{1}{2}i, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

where, as before, k = the radius of the coil, and n = the number of turns made by the wire round the coil.

The quantity in a given charge which can be continually reproduced under fixed conditions may be measured by allowing a succession of discharges to pass at regular and very short intervals through a galvanometer, so as to produce a permanent deflection. The value of a current producing this deflection can be ascertained; and the quotient of this value by the number of discharges taking place in the "second" gives the value of each charge in electromagnetic measure.

To find the dimensions of Q, we simply observe that the unit of electricity is that which is transferred by the unit current in the unit of time. Multiplying the dimensions of C by T, we find the dimensions of Q are $L^{\frac{1}{2}} M^{\frac{1}{2}}$.

26. *Electric Capacity of a Conductor.*—It is found by experiment that, other circumstances remaining the same, the charge on an insulated conductor is simply proportional to the electromotive force between it and the surrounding conductors, or, in other words, to the difference of potentials (47). The charge

* Weber calls this quantity two units—a fact which must not be lost sight of in comparing his results with those of the Committee.

that would be produced by the unit electromotive force is said to measure the electric capacity of a conductor. Thus, generally, the capacity of a conductor $S = \frac{Q}{E}$, where Q is the whole quantity in the charge produced by the electromotive force E. When the electromotive force producing the charge is capable of maintaining a current, the capacity of the conductor may be obtained without a knowledge of the value either of Q or E, provided we have the means of measuring the resistance of a circuit in electromagnetic measure. For let R be the resistance of a circuit, in which the given electromotive force, E, will produce the unit deflection on a tangent galvanometer, then, from equations (6) and (12), we have

$$S = 2\frac{t \sin \frac{1}{2} i}{\pi R_1}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

where t and i retain the same signification as in equation (13) (§ 25).

27. *Direct Measurement of Electromotive Force.*—The meaning of the words "electromotive force" has already been explained (§ 16). This force tends to do work by means of a current or transfer of electricity, and may therefore be said to produce and maintain the current. In any given combination in which electric currents flow, the immediate source of the power by which the work is done is said to produce the electromotive force. The sources of power producing electromotive force are various. Of these, chemical action in the voltaic battery, unequal distribution of temperature in circuits of different conductors, the friction of different substances, magnetoelectric induction, and simple electric induction are the most familiar. An electromotive force may exist between two points of a conductor, or between two points of an insulator, or between an insulator and a conductor, —in fine, between any points whatever. This electromotive force may be capable of maintaining a current for a long time, as in a voltaic battery, or may instantly cease after producing a current of no sensible duration, as when two points of the atmosphere at different potentials (§ 47) are joined by a conductor; but in every case in which a constant electromotive force, E, is maintained between any two points, however situated, the work spent or gained in transferring a quantity, Q, of electricity from one of those points to the other will be constant; nor will this work be affected by the manner or method of the transfer. If the electricity be slowly conveyed as a static charge on an insulated ball, the work will be spent or gained in accelerating or retarding the ball; if the electricity be conveyed rapidly through a conductor of small resistance, or more slowly through a con-

ductor of great resistance, the work may be spent in heating the conductor, or it may electrolyze a solution, or be thermoelectrically or mechanically used; but in all cases the change effected, measured as equivalent to work done, will be the same, and equal to EQ. Hence the electromotive force between two points is unity, if a unit of mechanical work is spent (or gained) in the transfer of a unit of electricity from one point to the other. This general definition is due to Professor W. Thomson.

The direct measurement of electromotive force would be given by the measure, in any given case, of the work done by the transfer of a given quantity of electricity. The ratio between the numbers measuring the work done, and the quantity transferred, would measure the electromotive force. This measurement has been made by Dr. Joule and Professor Thomson, by determining the heat developed in a wire by a given current measured as in (§ 18)*.

28. *Indirect Measurements of Electromotive Force.*—The direct method of measurement is in most cases inconvenient, and in many impossible; but the indirect methods are numerous and easily applied. The relation between the current, C, the resistance, R, and the electromotive force, E, expressed by Ohm's law (equation 6), will determine the electromotive force of a battery whenever R and C are known. A second indirect method depends on the measurement of the statical force with which two bodies attract one another when the given electromotive force is maintained between them. This method is fully treated in Part IV. (43). The phenomenon on which it is based admits of an easy comparison between various electromotive forces by electrometers. This method is applicable even to those cases in which the electromotive force to be measured is incapable of maintaining a current. The laws of chemical electrolysis and electromagnetic induction afford two other indirect methods of estimating electromotive force in special cases (54 and 31).

29. *Measurement of Electric Resistance.*—We have already stated that the resistance of a conductor is that property in virtue of which it limits the amount of work performed by a given electromotive force in a given time, and we have shown that it may be measured by the ratio $\frac{E}{C}$ of the electromotive force between two ends of a conductor to the current maintained by it. The unit resistance is therefore that in which the unit electromotive force produces the unit current, and therefore performs the unit of work, in the unit of time. If in any circuit

* Phil. Mag. S. 4. vol. ii. (1851), p. 551.

we can measure the current and electromotive force, or even the ratio of these magnitudes, we should, *ipso facto*, have measured the resistance of the circuit. The methods by which this ratio has been measured, founded on the laws of electromagnetic induction, are fully described in Appendix D (Brit. Assoc. Reports, p. 163). Other methods may be founded on the measurement of currents and electromotive forces described in 18, 19, 20, 27, and 28. Lastly, a method founded on the gradual loss of charge through very great resistances will be found in Part IV. (45). The equation (25) there given for electrostatic measure is applicable to electromagnetic measure when the capacity and difference of potentials are expressed in electromagnetic units.

30. *Electric Resistance in Electromagnetic Units is measured by an Absolute Velocity.*—The dimensions of R are found, by comparing those of E and C, to be $\frac{L}{T}$, or those of a simple velocity. This velocity, as was pointed out by Weber, is an absolute velocity in nature, quite independent of the magnitude of the fundamental units in which it is expressed. The following illustration, due to Professor Thomson, will show how a velocity may express a resistance, and also how that expression may be independent of the magnitude of the units of time and space.

Let a wire of any material be bent into an arc of $57\frac{1}{4}°$ with any radius, k. Let this arc be placed in the magnetic meridian of any magnetic field, with a magnet of any strength freely suspended in the centre of the arc. Let two vertical wires or rails, separated by a distance equal to k, be attached to the ends of the arc; and let a cross piece slide along these rails, inducing a current in the arc. Then it may be shown that the speed required to produce a deflection of 45° on the magnet will measure the resistance of the circuit, which is assumed to be constant. This speed will be the same whatever be the value of k, or the intensity of the magnetic field, or the moment of the magnet. In this form the experiment could not be easily carried out; but if a length, l, of wire be taken and rolled into a circular coil at the radius k, and the distance between the vertical rails be taken equal to $\frac{k^2}{l}$, then, if the resistance of the circuit be the same as in the previous case, the deflection of 45° will be produced by the same velocity in the cross piece, measuring that resistance; or, generally, if the distance between the rails be $p\frac{k^2}{l}$, then p times the velocity required to produce the unit deflection (45°) will measure the resistance. The truth of this

proposition can easily be established when the laws of magneto-electric induction have been understood (31).

31. *Magneto-electric Induction.*—Let a conducting circuit be placed in a magnetic field. Let C be the intensity of any current in that circuit; E the magnitude of the electromotive force acting in the circuit. Let the circuit be so moved that the number of lines of magnetic force (11) passing through it is increased by N in the time t, then (23) the electromagnetic forces will contribute towards the motion an amount of work measured by CN. Now Q, the quantity of electricity which passes, is equal to Ct; so that the work done on the current is EQ or CEt. By the principle of conservation of energy, the work done by the electromagnetic forces must be at the expense of that done by the electromotive forces, or

$$CN + CEt = 0;$$

or dividing by Ct, we find that

$$E = -\frac{N}{t}; \quad \ldots\ldots\ldots \quad (15)$$

or, in other words, if the number of lines of force passing through a circuit be increased, an electromotive force in the negative direction will act in the circuit measured by the number of lines of force added per second.

If R be the resistance of the circuit, we have by Ohm's law (equation 6) $E = CR$; and therefore

$$N = -Et = -RCt = -RQ; \quad \ldots\ldots \quad (16)$$

or, in other words, if the number of lines of magnetic force passing through the circuit is altered, a current will be produced in the circuit in the direction opposite to that of a current which would have produced lines of force in the direction of those added, and the quantity of electricity which passes, multiplied by the resistance of the circuit, measures the number of additional lines passing through the circuit.

The facts of magneto-electric induction were discovered by Faraday, and described by him in the First Series of his "Experimental Researches in Electricity," read to the Royal Society, November 24th, 1831.

He has shown* the relation between the induced current and the lines of force cut by the circuit, and he has also described the state of a conductor in a field of force as a state the change of which is a cause of currents. He calls it the electrotonic state; and, as we have just seen, the electrotonic state may be *measured* by the number of lines of force which pass through the circuit at any time.

* Experimental Researches, 3082, &c.

The measure of electromotive force used by W. Weber, and derived by him (independently of the principle of conservation of energy) from the motion of a conductor in a magnetic field, is the same as that at which we have arrived; for, from equation (15), we find that the unit electromotive force will be produced by motion in a magnetic field when one line of force is added (or subtracted) per unit of time; and this will occur when in a field of unit intensity a straight bar of unit length, forming part of a circuit otherwise at rest, is moved with unit velocity perpendicularly to the lines of force and to its own direction.

To W. Weber, whose numerical determinations of electrical magnitudes are the starting-point of exact science in electricity, we owe this, the first definition of the unit of electromotive force; but to Professor Helmholtz* and to Professor W. Thomson†, working independently of each other, we owe the proof of the necessary existence of magneto-electric induction, and the determination of electromotive force on strictly mechanical principles.

32. *On Material Standards for the Measurement of Electrical Magnitudes.*—The comparison between two different electrical magnitudes of the same nature, *e. g.* between two currents or between two resistances, is in all cases much simpler than the direct measurement of these magnitudes in terms of time, mass, and space, as described in the foregoing pages. Much labour is therefore saved by the use of standards of each magnitude; and the construction and diffusion of those standards form part of the duties of the Committee.

Electric currents are most simply compared by "electro-dynamometers" (20)—instruments which, unlike galvanometers, are practically independent of the intensity of the earth's magnetism. When an instrument of this kind has been constructed, with which the values of the currents corresponding to each deflection have been measured (19), (20), other instruments may easily be so compared with this standard that the relative value of the deflections produced by equal currents on the standard and the copies shall be known. Hence the absolute value of the current indicated by each deflection of each copy will be known in absolute measure. In other words, in order to obtain the electromagnetic measure of a current in the system described, each observer in possession of an electro-dynamometer which has been compared with the standard instrument will simply multiply by a constant number the deflection produced by the current on his instrument (or the tangent or sine of the deflection, according to the particular construction of the instrument).

* Paper read before the Physical Society of Berlin, 1847 (*vide* Taylor's Scientific Memoirs, part 2. Feb. 1853, p. 114).

† Reports of the British Association, 1848; Phil. Mag. Dec. 1851.

Electric quantities may be compared by the swing of the needle of a galvanometer of any kind. They may be measured by any one in possession of a standard electro-dynamometer, or resistance-coil, since the observer will then be in a position directly to determine C_1 in equation (12), or R_1 in equation (14).

Capacities may be compared by the methods described (26); and a Leyden jar or condenser (41) of unit capacity, and copies derived from it, may be prepared and distributed. The owner of such a condenser, if he can measure electromotive force, can determine the quantity in his condenser.

The material standard for *electromotive force* derived from electromagnetic phenomena would naturally be a conductor of known shape and dimensions, moving in a known magnetic field. Such a standard as this would be far too complex to be practically useful: fortunately a very simple and practical standard or gauge of electromotive force can be based on its statical effects, and will be described in treating of those effects (Part IV. 43). A practical standard for approximate measurements might be formed by a voltaic couple, the constituent parts of which were in a standard condition. It is probable that the Daniell's cell may form a practical standard of reference in this way, when its value in electromagnetic measure is known. This value lies between 9×10^7 and 11×10^7.

Resistances are compared by comparing currents produced in the several conductors by one and the same electromotive force. The unit resistance, determined as in Appendix D (Brit. Assoc. Reports, p. 163), will be represented by a material conductor; simple coils of insulated wire compared with this standard, and issued by the Committee, will allow any observer to measure any resistance in electromagnetic measure.

[To be continued.]

LXII. *On a Theorem relating to Five Points in a Plane.* By A. Cayley, *F.R.S.**

TWO triangles, ABC, A′B′C′ which are such that the lines AA′, BB′, CC′ meet in a point, are said to be in perspective; and a triangle A′B′C′, the angles A′, B′, C′ of which lie in the sides BC, CA, AB respectively, is said to be inscribed in the triangle ABC; hence, if A′, B′, C′ are the intersections of the sides by the lines AO, BO, CO respectively (where O is any point whatever), the triangle A′B′C′ is said to be perspectively inscribed in the triangle ABC, viz. it is so inscribed by means of the point O.

We have the following theorem, relating to any triangle

* Communicated by the Author.

ABC, and two points O, O′. If in the triangle ABC, by means of the point O, we inscribe a triangle A′B′C′, and in the triangle A′B′C′, by means of the point O′, we inscribe a triangle $\alpha\beta\gamma$, then the triangles ABC, $\alpha\beta\gamma$ are in perspective, viz. the lines Aα, Bβ, Cγ will meet in a point.

This is very easily proved analytically; in fact, taking $x=0$, $y=0$, $z=0$ for the equations of the lines B′C′, C′A′, A′B′ respectively, and (X, Y, Z) for the coordinates of the point O, then the coordinates of (A, B, C) are found to be $(-X, Y, Z)$, $(X, -Y, Z)$, $(X, Y, -Z)$ respectively. Moreover, if (X′, Y′, Z′) are the coordinates of the point O′, then the coordinates of (α, β, γ) are found to be

$$(0, Y', Z'),\ (X', 0, Y),\ (X', Y', 0)$$

respectively. Hence the equations of the lines Aα, Bβ, Cγ are respectively

$$\begin{vmatrix} x, & y, & z \\ -X, & Y, & Z \\ 0, & Y', & Z' \end{vmatrix} = 0, \quad \begin{vmatrix} x, & y, & z \\ X, & -Y, & Z \\ X', & 0, & Z' \end{vmatrix} = 0, \quad \begin{vmatrix} x, & y, & z \\ X, & Y, & -Z \\ X', & Y', & 0 \end{vmatrix} = 0;$$

that is,

$$\begin{aligned} x(\ YZ'-Y'Z)+y(\ \ Z'X)+z(-XY'\ \ )&=0,\\ x(-YZ'\ \ )+y(\ ZX'-Z'X)+z(\ \ X'Y)&=0,\\ x(\ \ +Y'Z)+y(-ZX'\ \ )+z(\ XY'-X'Y)&=0, \end{aligned}$$

which are obviously the equations of three lines which meet in a point.

But the theorem may be exhibited as a theorem relating to a quadrangle 1234 and a point O′; for writing 1, 2, 3, 4 in place of A, B, C, O, the triangle A′B′C′ is in fact the triangle formed by the three centres 41.23, 42.31, 43.12 of the quadrangle 1234, hence the triangle in question must be similarly related to each of the four triangles 423, 431, 412, 123; or, forming the diagram

	P	Q	R	S
41.23	4	3	2	1
42.31	3	4	1	2
43.12	2	1	4	3,

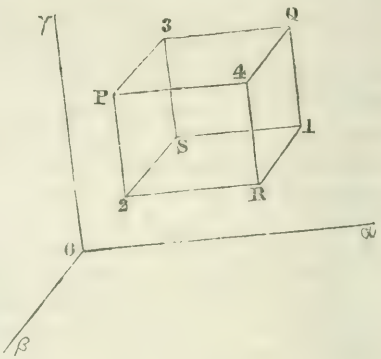
we have the following form of the theorem: viz. the lines

$\alpha 4$, $\beta 3$, $\gamma 2$ meet in a point P,
$\alpha 3$, $\beta 4$, $\gamma 1$ „ „ Q,
$\alpha 2$, $\beta 1$, $\gamma 4$ „ „ R,
$\alpha 1$, $\beta 2$, $\gamma 3$ „ „ S,

or, what is the same thing, we have with the points 1, 2, 3, 4 and the point O′ constructed the four points P, Q, R, S such that

1S, 2R, 3Q, 4P meet in α,
2S, 1R, 4Q, 3β „ β,
3S, 4R, 1Q, 2P „ γ.

The eight points 1, 2, 3, 4, P, Q, R, S form a figure such as the perspective representation of a parallelopiped, or, if we please, a cube; and not only so, but the plane figure is really a certain perspective representation of the cube; this identification depends on the following two theorems:—



1. Considering the four summits 1, 2, 3, 4, which are such that no two of them belong to the same edge, then, if through any point O we draw

the line OA′ meeting the lines 41, 23,
„ OB′ „ „ 42, 31,
„ OC′ „ „ 43, 12,

and the lines Oα, Oβ, Oγ parallel to the three edges of the cube respectively, the three planes (OA′, Oα), (OB′ Oβ), (OC′, Oγ) will meet in a line.

2. For a properly-selected position of the point O,

the lines OB′, OC′, Oα will lie in a plane,
„ OC′, OA′, Oβ „ „ ,
„ OA′, OB′, Oγ „ „ .

In fact for such a position of O, projecting the whole figure on any plane whatever, the lines O1, O2, O3, O4, OP, OQ, OR, OS, Oα, Oβ, Oγ, OA′, OB′, OC′ meet the plane of projection in the points 1, 2, 3, 4, P, Q, R, S, α, β, γ, A′, B′, C′ related to each other as in the last-mentioned form of the plane theorem. To prove the two solid theorems, take O for the origin, Oα, Oβ, Oγ for the axes, (α, β, γ) for the coordinates of the summit S, and 1 for the edge of the cube,

the coordinates of 1 are $\alpha+1$, β, γ,
„ 2 „ α, $\beta+1$, γ,
„ 3 „ α, β, $\gamma+1$,
„ 4 „ $\alpha+1$, $\beta+1$, $\gamma+1$.

The equations of the line OA′, or say of the line O(41, 23), are those of the planes O 41, O 23, viz. these are

$$\begin{vmatrix} x & , y & , z \\ \alpha+1, & \beta & , \gamma \\ \alpha+1, & \beta+1, & \gamma+1 \end{vmatrix} = 0, \quad \begin{vmatrix} x, & y & , z \\ \alpha, & \beta+1, & \gamma \\ \alpha, & \beta & , \gamma+1 \end{vmatrix} = 0;$$

that is,

$$x(\beta-\gamma) - (\alpha+1)(y-z) = 0,$$
$$x(\beta+\gamma+1) - \alpha(y+z) = 0.$$

Writing for shortness

$$M = \alpha+\beta+\gamma+1,$$

these equations give

$$x : y : z = \frac{2\alpha(\alpha+1)}{(M+2\gamma\alpha)(M+2\alpha\beta)} : \frac{1}{M+2\gamma\alpha} : \frac{1}{M+2\alpha\beta};$$

or completing the system,

for line OA′ we have

$$x : y : z = \frac{2\alpha(\alpha+1)}{(M+2\gamma\alpha)(M+2\alpha\beta)} : \frac{1}{M+2\gamma\alpha} : \frac{1}{M+2\alpha\beta};$$

for line OB′ we have

$$x : y : z = \frac{1}{M+2\beta\gamma} : \frac{2\beta(\beta+1)}{(M+2\alpha\beta)(M+2\beta\gamma)} : \frac{1}{M+2\alpha\beta};$$

for line OC′ we have

$$x : y : z = \frac{1}{M+2\beta\gamma} : \frac{1}{M+2\gamma\alpha} : \frac{2\gamma(\gamma+1)}{(M+2\beta\gamma)(M+2\gamma\alpha)}.$$

The equations of the lines Oα, Oβ, Oγ are of course $(y=0, z=0)$, $(z=0, x=0)$, $(x=0, y=0)$ respectively; and we therefore see at once that the planes (OA′, Oα), (OB′, Oβ), (OC′, Oγ) meet in a ine, viz. in the line which has for its equations

$$x : y : z = \frac{1}{M+2\beta\gamma} : \frac{1}{M+2\gamma\alpha} : \frac{1}{M+2\alpha\beta}.$$

The lines OB′, OC′, Oα will lie in a plane, if only

$$1 = \frac{4\beta\gamma(\beta+1)(\gamma+1)}{(M+2\beta\gamma)^2};$$

that is,

$$(M+2\beta\gamma)^2 = 4\beta\gamma(\beta+1)(\gamma+1),$$

or, as this may be written,

$$M^2 + 4\beta\gamma(\alpha+\beta+\gamma = 1+\beta\gamma) = 4\beta\gamma(\beta\gamma+\beta+\gamma+1);$$

that is,

$$M^2+4\alpha\beta\gamma=0,$$

or, what is the same thing,

$$(\alpha+\beta+\gamma+1)^2+4\alpha\beta\gamma=0;$$

and from the symmetry of this equation we see that when it is satisfied

the lines OB′, OC′, Oα will lie in a plane,
„ OC′, OA′, Oβ „ „
„ OA′, OB′, Oγ „ „

viz. this will be the case when the point O is situate in the cubic surface represented by the last-mentioned equation; this completes the demonstration of the solid theorems.

It is clear that considering five points 1, 2, 3, 4, 5 in a plane, then, since any one of these may be taken for the point O′ of the foregoing theorem, the theorem exhibited in the first instance as a theorem relating to a triangle and two points, and afterwards as a theorem relating to a quadrangle and a point, is really a theorem relating to five points in a plane. There are, of course, five different systems of points (P, Q, R, S), corresponding to the different combinations of four out of the five points.

Cambridge, March 6, 1865.

LXIII. *On Chemical Nomenclature.*

By Alexander W. Williamson, *F.R.S., F.C.S.**

I HAD some weeks ago the honour of submitting to the consideration of the Chemical Society a few practical suggestions on the subject of chemical nomenclature, framed in the hope of diminishing the inconsistencies which prevail in it at present, and of aiding the development of its best tendencies.

My chief proposal was to adopt, as systematically as possible, terms such as mercurous nitrate, $Hg^2(NO^3)^2$; mercuric hydronitrate, Hg HO NO^3; hydric sulphate, H^2SO^4; potassic hydrate, KHO; hydropotassic sulphate, $HKSO^4$; hydrodisodic phosphate, H Na^2PO^4; sodic sulphate, Na^2SO^4; sodic disulphate, $Na^2S^2O^7$, &c.; ferric oxydisulphate, $Fe^2O(SO^4)^2$; ferric dioxysulphate, $Fe^2O^2SO^4$, &c. The result of two evenings' discussion of the subject was to show that the principles of such nomenclature are, upon the whole, approved, and the names formed in accordance with those principles offer altogether greater proportional recommendations than any other names which are before chemists.

* Communicated by the Author.

In the course of the discussion which took place on the subject, I had occasion to point out that, inasmuch as salts in which the base is hydrogen, such as hydric nitrate, hydric sulphate, hydric phosphate, &c., are admitted to be analogous in their constitution and properties to the salts of the regular metals, such as silver, potassium, &c., it is desirable, when describing their reactions, to designate them by names bearing a corresponding analogy to the names of the salts of silver, potassium, &c.; that in describing the reactions of double salts containing as base partly hydrogen, partly some heavier metal, such as common rhombic phosphate, $H\,Na^2\,PO^4$, it is not only desirable to introduce the name of the hydrogen in a form similar to that of the other metal, but it is really not possible to obtain systematic and consistent names without representing in them the metallic functions of the hydrogen; that when hydrogen is in the place of an acid or chlorous constituent of a salt, it must be described by a term which represents the fact of its having such functions.

In fact it is not allowable to apply to hydrogen-salts names which conceal their analogy with other salts, or which imply the absence of saline constitution in hydrogen-salts. Thus it is a faulty expression to say that the common process for preparing so-called nitric acid consists in the action of sulphuric acid on potassic nitrate, forming potassic bisulphate and nitric acid; for such an expression conveys the idea of a mere displacement of one acid by another, whereas the process is admitted to be an interchange of half the hydrogen in hydric sulphate with potassium in potassic nitrate, forming hydropotassic sulphate and hydric nitrate.

It was admitted by all who spoke on the subject at the Chemical Society, that hydrogen-salts must in exact language be named similarly to other salts; and one distinguished member mentioned that, in describing to students such a reaction as the above, he uses such terms as sulphate of hydrogen and nitrate of hydrogen.

It was at first supposed by some members that I advocated the immediate introduction of systematic and accurate names into common and popular language. The learned member felt alarm at the danger of having to speak of mercurous chloride instead of "calomel," manganic peroxide instead of "manganese," hydric sulphate instead of "sulphuric acid," &c.; and manufacturers would certainly not have received with favour a proposal to give up the term "soda" for sodic carbonate, to say arsenious acid instead of "arsenic."

I accordingly hastened to explain that my suggestions towards improving our systematic nomenclature were only expected, if adopted, to react gradually upon the popular language, and that

for the present I contemplate ordering a couple of carboys of "sulphuric acid" or "nitric acid" as heretofore, meaning those compounds which in systematic language are designated "hydric sulphate" and "hydric nitrate"; but that when I have to explain to learners the reactions of those hydrogen-salts, I should give them the systematic names which correspond to their composition. The popular and trivial names by which they are known are abbreviations formed so as to point to the essential or characteristic constituent. It is not practicable to send out real sulphuric acid, SO^3; but manufacturers and consumers know that the value of oil of vitriol is not in the water which it contains, but in the "real acid." In like manner, the common crystals of hydrated sodic carbonate are valuable in proportion to the percentage of soda, Na^2O, which they contain, and they are not unreasonably named after their characteristic constituent.

There was, on the part of one or two distinguished members of the Society, a feeling that the retention of the words acid and base in their established signification of "electro-negative oxides" and "electro-positive oxides" might be inconvenient in presence of the fact that chlorine forms with hydrogen a very acid salt, and that some other elements also form acid hydrides. But when it is admitted that H^2SO^4 is a salt, though of very acid properties, that HNO^3 and H^3PO^8 are also very acid salts, and that in scientific language they must be designated as salts, it really is not surprising that HCl, HBr, &c. should be salts of considerable acidity, and it is not unnatural to call them salts of hydrogen in systematic nomenclature. The fact that we cannot remove the elements of water from hydric chloride and make Cl^2-O, whilst we can remove water from hydric sulphate and make SO^4-O, is really no reason against classing, side by side, hydrogen-salts with compound radicals such as NO^3, SO^4, PO^4, &c., and those with elementary radicals such as Cl, Br, &c.

Since my suggestions have been published Mr. Foster has published in the Philosophical Magazine a paper "On Chemical Nomenclature, and chiefly on the use of the word Acid." In this paper Mr. Foster expresses assent to the form of names of which I had recommended the systematic adoption; and he says, "If we regard the salts of hydrogen as constituted like the salts of any other metal, the application to them of the name acid becomes incorrect if it implies any peculiarity of constitution, and superfluous if it does not." Now, as Laurent and Gerhardt did admit and assert that the salts of hydrogen are constituted like the salts of any other metal, and as Mr. Foster is doubtless perfectly aware that they did so, the above sentence is a distinct condemnation of Gerhardt's proposal of applying the word acid

to salts with hydrogen as base. And coupled, as it is, with Mr. Foster's admission that these hydrogen-salts ought, in systematic language, to be called hydric sulphate, hydric nitrate, &c., it does convey Mr. Foster's assent in a very full manner to the principle of the proposal which I made on the subject of Nomenclature.

The general form of Mr. Foster's paper is, however, that of an argument against my proposal; and the paper contains some statements to which my silence would probably seem to give a consent, which I really cannot give. It must have been from inadvertence that Mr. Foster speaks of my *wishing to apply* the name acid to such bodies as CO^2, SO^3, SiO^2, &c.; for I merely remarked that the name that belongs to them is wanted by its owners, and that it does not suit the hydrogen-salts to which Gerhardt wanted to transfer it.

Mr. Foster goes into an elaborate exposition of what he conceives to be the original meaning of the word acid, and speaks of that "original meaning" as "anything but particularly clear." He might safely have called it "particularly cloudy."

Every chemist knows that the great Berzelius epitomized the prevailing definition by saying that an acid is an electro-negative oxide, and a base is an electro-positive oxide. No definition is complete and perfect; but this definition is certainly clear, and it does point to differences of properties among chemical compounds which are the most characteristic and important known to us. I cannot see any chance whatever of the words acid and base being given up; for they describe conveniently the chief differences of properties by which we classify compounds chemically. Mr. Foster's remark, that "the strictly scientific significance of the word acid has passed away," and that the word indicates "a distinction to which we now know that no real difference corresponds," must be taken as referring to Gerhardt's misuse of the word acid, as describing salts with basic hydrogen. He might have gone a step further in condemnation of that misuse of the word, and have shown that the word acid never has had any scientific significance as applied to hydrogen-salts.

Mr. Foster quotes from my note (but apparently misunderstands) the statement, "In fact he [Gerhardt] systematically applied the term acid to hydrogen-salts, giving the name anhydride to acids, and leaving bases, however anhydrous they might be, entirely unprovided with a corresponding name." If bodies such as HNO^3, H^2SO^4, H^3PO^4 were considered to be entitled to the name "acid," then for precisely similar reasons, bodies such as KOH, $Ba(OH)^2$ would be entitled to the name "base"; and if the bodies N^2O^5, SO^3, P^2O^5, &c. formed by dehydrating these so-called acids are called "anhydrides," then some corresponding and

distinctive name should be given to the bodies K^2O, BaO, &c., formed by dehydrating the so-called bases. The absence of any such term is a *deficiency* sufficiently grave to make one pause in adopting the term anhydride in systematic language, until the idea which it represents is duly applied to the other great class of chemical compounds; but I cannot, with Mr. Foster, call it a "limitation"; and as I have not said that Gerhardt imposed any "limitation" in the matter, I may fairly be excused from accepting Mr. Foster's challenge to show *where* Gerhardt imposed it. If Mr. Foster were to deny my statement that the anhydrous bases are unprovided by Gerhardt with a name corresponding to that of anhydride for the acids, I might probably beg the favour of his quoting chapter and verse in support of his denial. But as matters now stand, the two great classes of chemical compounds are called acids (such as CO^2, SO^3, SiO^2, &c.) and bases (such as K^2O, CaO, Fe^2O^3, &c.). Whoever wants to take their names from them for the use of their hydrates must at least give them new names which will do as well. And he will certainly not be permitted to take the two names from the two classes of bodies, and put them off with one name between them. Gerhardt seems to have thought that he would be permitted to do so, but the single substitute (anhydride) which he offered is admitted to be not only insufficient but absolutely unacceptable.

Perhaps the most important advantage which chemists have gained by representing all substances of known composition by typical formulæ, has been the increased clearness with which they have been able to compare the properties of bodies with one another, without the mind being encumbered by conventional differences of form. Even elements are now for the most part represented by formulæ analogous to those used in representing compounds; free hydrogen being HH like HCl, free oxygen being OO like CaO, free phosphorus being P^3P like H^3N, &c. The one great difference which stands forth above all other chemical differences, is that which is described in various terms, all more or less similar in import to acid or acid-like and basic or base-like. We have long since admitted that this fundamental difference is a difference in the degree in which various substances exert analogous effects, a weak acid acting like a base under the influence of a very strong acid, and a weak base acting like an acid to a very strong base. Among simple and well-known compounds this difference is most markedly represented by oxides such as SO^3, P^2O^5, SO^2, CO^2, CaO, K^2O, PbO, Bi^2O^3, &c.; and every chemist knows that compounds of the former class are electro-negative to those of the latter class, electro-negative oxides being called acids, and electro-positive oxides being called bases. It is admitted that hydrogen-salts

must be represented and named like other salts; hydric nitrate, or hydric phosphate like potassic nitrate or potassic phosphate, and potassic hydrate or calcic hydrate like potassic nitrate or calcic nitrate; and Gerhardt's attempt to apply to bodies of the first class the name acid is, in the words of Mr. Foster, "incorrect if it implies any peculiarity of constitution [different from other salts], and superfluous if it does not." Mr. Foster might, however, as above remarked, have added that Gerhardt's definition of the word acid is simply in itself devoid of meaning. He quotes it thus: Acids are "salts whose *base* [the italics are mine] is wholly composed of hydrogen." A person ignorant of the meaning of the words acid and base could surely not ascertain from his inner consciousness which is the acid and which the basic constituent in any of the following compounds, KOH, HNO^3, BaO^2H^2, SO^4H^2; and Gerhardt's pretended definition would afford him no aid in ascertaining which of these compounds are to be called acids, which bases. One is almost tempted to suspect Mr. Foster of bitter irony when he calls this definition "strictly scientific and logical." Although different in form, it is not one bit more reasonable than the Munchausen (or Irish ?) feat of ascending to the moon by the aid of a mile-long chain, the traveller first fastening his chain by one end at a point one mile up, then climbing up by the chain to that point, and so on. But I am sorry to say that Gerhardt's disciple is even in a worse plight than the aëronaut, he is so unfortunately circumstanced that even if his chain were fastened one mile up, he could not climb up it. For if, as a preliminary to the understanding of Gerhardt's dictum, we are told how to find out which is the acid and which the basic constituent of a given compound, we find that this preliminary information is inconsistent with Gerhardt's dictum, and prevents our making any use of it. By the aid of a battery anybody could find out which are electro-negative, which the electro-positive oxides derivable from the above compounds; but Gerhardt would then reject the result as inapplicable to his purpose.

It has always seemed to me that the most plausible objection to the use of the terms acid and base in the sense of electro-negative oxide and electro-positive oxide was the fact that some acids, such as SO^3, P^2O^5, SiO^2, &c., may be put in contact with bases such as BaO, K^2O, &c. without manifesting any strong tendency to combine with them; and observations of this kind led some chemists to say that, in their chemical properties, these so-called acids do not behave like acids, and that it is therefore reasonable to deprive them of the name acid. Now the fact is that these acids always do combine with bases when brought in contact with them in the fluid state, and they combine with more force

than that with which their hydrates react on basic hydrates. It is well known that when two saline molecules such as SO^4H^2 and BaO^2H^2 react on one another with liberation of water and formation of a salt, the force of combination, as measured by the heat evolved, is less than that which the acid and base exert in direct combination; for the process of double decomposition separates the water from SO^3 and from BaO, and in doing so absorbs just as much heat as was evolved when water combined with SO^3 and with BaO; so that the force with which the two hydrates react on one another is by so much less than that with which SO^3 combines with BaO.

Mr. Foster expresses an objection to applying the term "combination" to the reaction of such bodies as anhydrous acetic acid $(C^2H^3O)^2O$ on water, because by a process of double decomposition the two molecules, acid and water, give rise to the formation of two new molecules; but if his objection is admitted to have weight, it applies equally to the reaction of free chlorine on free hydrogen, where two molecules of the elements form two molecules of the compound by a process of double decomposition. If such reactions as that of chlorine on hydrogen, and of anhydrous acetic acid on water, are not combinations, the word might perhaps be retained for such reactions as the combination of carbonic oxide and chlorine; or SO^3 and water, where two molecules unite to form one; but if Mr. Foster seriously proposes such a restriction of the word, it will be time enough to consider it. The present usage is to describe as combinations those reactions in which the resulting molecules are less various than the original molecules, as in the cases of

$$O^2+(H^2)^2=(H^2O)^2;\quad Cl^2+K^2=(ClK)^2;$$
$$N^2O^5+H^2O=(NO^3H)^2;\ (C^2H^3O)^2O+H^2O=(C^2H^4O^2)^2,\&c.;$$

and, in like manner, to describe as decompositions those reactions in which the products are more various than the materials, as

$$SO^4H^2=SO^3+H^2O;\ Pb(NO^3)^2=PbO+O+N^2O^4;$$
$$(C^2H^3KO^2)^2=C^3H^6O+CO^3K^2;\quad (HgO)^2=2Hg+O^2,$$
$$\begin{pmatrix}C^2H^5\\H\end{pmatrix}O\Big)^2+SO^4H^2=\begin{pmatrix}C^2H^5\\C^2H^5\end{pmatrix}O\Big)+H^2O+SO^4H^2.$$

Many of these processes are known to consist of a series of double decompositions, and the fact is often mentioned in alluding to them; but it does not seem likely that we should abandon the use of the terms combination and decomposition.

Mr. Foster has discussed in his paper what he calls the original use of the words acid and base, which is sufficiently characterized by his own words, "anything but particularly clear."

He has also discussed Gerhardt's misuse of the word acid. His conclusion that the word had better be given up, would be quite worthy of serious consideration if the words were only used in those improper senses. But the words acid and base really mean something not only true, but of fundamental importance, which we are constantly obliged to consider and speak of in chemistry; and I am quite sure that it would be utterly beyond my power to take from them their established meaning, even if I wished to do so. There is at present a considerable amount of inconsistency in the prevailing use of these, as of most other scientific terms; and Mr. Foster's interesting paper affords further arguments than those which I had given in favour of abandoning as speedily as practicable the misuse of the terms which has crept into partial use through popular disregard of water in hydric sulphate, and which Gerhardt unsuccessfully endeavoured to incorporate with scientific language.

I have not discussed the proposal to call both acids and bases oxides, because it has not as yet received sufficient development to enable me to form any opinion upon it beyond the obvious objections which present themselves at first sight to so grave a change. Thus MnO, Mn^2O^3, MnO^2, MnO^3, Mn^2O^7 are at present conveniently distinguished by names, calling the last two acids, and the first two oxides; and so also CO and CO^2 are very conveniently distinguished by the words oxide and acid. Another circumstance which would alone have been sufficient to prevent my offering any opinion on this proposal is the fact above explained, that it is founded on Mr. Foster's opinion that the word acid is not clear and is unworthy of being retained.

University College, London,
May 16, 1865.

LXIV. *On the Absorption of Light at different Temperatures.* By M. Feussner*.

THE prismatic examination of light which has passed through absorbent media has been continually acquiring greater importance ever since Stokes called attention to its practical utility. In particular it is interesting to examine the alterations in absorption which take place on mixing two absorbing substances which exert no chemical action upon each other, and the alterations caused by changes of temperature.

Professor Melde of Marburg has described† the modifications

* From the *Monatsber. d. königl. preuss. Akad. d. Wissensch. z. Berlin*, March 30, 1865, p. 144.

† Poggendorff's *Annalen*, vol. cxxiv. p. 91.

produced in the position of the absorption-bands of a solution of carmine by mixing it with other coloured solutions, and was thus the first to call attention to phenomena of this nature. The following observations, on analogous phenomena presented by indigo, had been already made by the author at the date of Professor Melde's communication.

It is well known that indigo gives a spectrum in which a comparatively narrow red band is followed by an absorption-band, of greater or less breadth according to the concentration of the solution, while after this there comes a bright band which attains its maximum of intensity in the blue, and lastly the violet end of the spectrum again suffers absorption. Now if a small quantity of a solution of sulphate of copper is mixed with such a solution of indigo, the red line disappears immediately, and after a short time the second bright band begins to approach the red end of the spectrum, and finally extends in this direction to the extent of about one-eighth of the breadth of the entire spectrum. The indigo solution, however, must not contain any free sulphuric acid; for if only one drop of this acid is added to it the first spectrum reappears. It thus becomes a question whether the change above described does not depend on the formation of a new chemical compound. On adding dichromate of potassium to the indigo instead of sulphate of copper, a much smaller alteration takes place. The red band then remains unchanged, and after adding several drops we observe only a displacement of the limits of the green towards the red end, the maximum displacement amounting to about one-thirtieth of the total breadth of the spectrum.

The solutions examined by the author with respect to alterations of absorption caused by changes of temperature, were ferric chloride, cupric chloride, cupric sulphate, sulphate of cuprammonium, dichromate of potassium, sesquinitrate of nickel, protochloride of cobalt, and dichloride of platinum. In all these an alteration was manifest, namely in all cases an increase of absorbing power with rise of temperature; but this was much greater in the case of chlorides than with the other salts. Chloride of copper, for instance, when employed at the proper degree of concentration, becomes completely opake at the boiling-point. And it is worthy of remark here, that the part of the spectrum which remains longest visible as the temperature is raised, does not coincide exactly with the part which is the last to disappear when the thickness of the stratum is gradually increased; so that the point of maximum intensity of the spectrum comes at a different place in the heated substance from that which it occupies at ordinary temperatures.

The behaviour of chloride of cobalt is also interesting. At

common temperatures and at the proper degree of concentration this substance shows two luminous bands, one of which is very intense and embraces the whole of the red and yellow, and part of the green; the other, comparatively weak, is situated in the violet. On applying heat, this violet band gradually diminishes in intensity, and two new bands of absorption, of which previously no trace was visible, appear in the red. They increase very rapidly in breadth, especially the less refrangible of the two, as the temperature rises; so that, when the boiling-point is approached, they have completely obliterated the entire bright band in which they appeared, with the exception of a very narrow weak stripe in the extreme red.

In order to explain these phenomena, one might be disposed to assume that the elevation of temperature occasioned chemical changes to take place in the liquids—that, for instance, a few atoms of water were fixed or given off—were it not that, so far as the observations have yet gone, a sudden alteration of absorbing-power never occurs, but the changes take place in a perfectly gradual manner.

On the other hand, these phenomena are quite analogous to those observed by Brewster* and others in relation to the absorbing-powers of certain gases, in which, as the temperature rises, the absorption-bands increase in number and width.

LXV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 398.]

Feb. 23, 1865.—John P. Gassiot, Esq., Vice-President, in the Chair.

THE following communications were read:—

"On New Cornish Minerals of the Brochantite Group." By Professor N. Story Maskelyne, M.A., Keeper of the Mineral Department, British Museum.

On a small fragment of Killas from Cornwall, a discovered, several months ago, a new mineral in the form of minute but well-formed crystals. The specimen had come from Mr. Talling, of Lostwithiel, a mineral-dealer, to whose activity and intelligence I am indebted for the materials that form the subject of this paper. After a little while he found the locality of the mineral, and sent me other and finer specimens; but these specimens proved to contain other new minerals besides the one already mentioned. Two of these minerals are described in this paper, and a third will form the subject of a further communication.

I. *Langite.*

The first of these minerals which I proceed to describe is one to

* Phil. Mag. S. 3. vol. viii. p. 386.

which I have given the name of Langite, in honour of my friend Dr. Viktor von Lang, now of Gratz, and lately my colleague in the British Museum. It occurs in minute crystals, or as a crystalline crust on the Killas, of a fine blue with a greenish hue in certain lights. The crystals are prismatic. The forms observed are (1 0 0), (0 0 1), (1 1 0), and (2 0 1) & (0 1 0), the normal inclinations giving the following angles, which are the averages of many measurements :—

$$1\,1\,0 \wedge \bar{1}\,1\,0 = 56^\circ\ 16'$$
$$1\,0\,0 \wedge 1\,1\,0 = 61\ 52$$
$$0\,0\,1 \wedge 2\,0\,1 = 51\ 46$$

conducting to the parametral ratios

$$a : b : c = 1 : 0{\cdot}5347 : 0{\cdot}6346.$$

The crystals are twinned after the manner of cerussite, the twin axis being normal to the plane (1 1 0).

$$\bar{1}\,1\,0 \wedge (1\,1\,0) \wedge \bar{1}\,1\,0 = 112^\circ\ 33'$$
$$1\,0\,0 \wedge (1\,1\,0) \wedge 1\,0\,0 = 123\ 44$$
$$\bar{1}\,1\,0 \wedge (1\,1\,0) \wedge 1\,\bar{1}\,0 = 67\ 26$$

Cleavages seem to exist parallel to 0 0 1 and 1 0 0. The planes 0 0 1 and 1 0 0 are very brilliant. The plane of the optic axes, as seen through a section parallel to the plane 0 0 1, is parallel to 1 0 0. The normal to 0 0 1 would seem to be the first mean line, and it is negative. The optical orientation of the mineral is therefore b, c, a.

The crystals are dichroic.

1. Seen along axis *c*, c, greenish blue.
   b, blue.
2. Seen along axis *a*, c, darker greenish blue.
   a, lighter bluish green.

The specific gravity of Langite is 3·48 to 3·50. Its hardness is under 3. It will not abrade calcite.

Before the blowpipe on charcoal it gives off water, and fumes and becomes reduced to metallic copper. Insoluble in water, it is readily dissolved by acids and ammonia. Heated, it passes through (1) a bright green, and (2) various tints of olive-green, till (3) it becomes black. Water is given off the whole time, and finally it has a strongly acid reaction.

The first stage corresponds to the loss of one equivalent of water; the second reduces its composition to that of Brochantite; at the third it loses all its water.

The chemical composition of Langite is represented by the formula $3Cu''H'_2O_2 + Cu''SO_4 + 2H'_2O$, which requires the following numbers :—

	Calculated percentage.		Average found.
4 equivalents of copper	126·72	=52·00	52·55
4 equivalents of oxygen	32·	=13·13	13·27
1 equivalent of sulphuric anhydride	40·	=16·41	16·42
5 equivalents of water	45·	=18·46	18·317
	243·72	100·00	100·56

I have met with a small and old specimen of Connellite with a twin crystal of Langite associated with it.

II. *Waringtonite.*

To a Cornish mineral associated with Langite, emerald to verdigris-green in colour, occurring in incrustations generally crystalline, and seen occasionally in distinct individual crystals aggregated loosely on the Killas, I have given the name of Waringtonite, in honour of my friend Mr. Warington Smyth. The crystals are always of the same form, that, namely, of a double-curved wedge. A narrow plane, 0 0 1, is very brilliant and without striation. It appears to be a cleavage-plane. A second, but scarcely measurable plane, 1 0 0, occurs at right angles to it, truncating the thin ends of the wedge. The prism planes in the zones 0 1 0, 0 0 1, and 0 1 0, 1 0 0 are uniformly curved. The planes of two prisms seem to exist in the zone 0 1 0, 0 0 1, but the angles, as approximately measured by the goniometer, are not very reliable; one of them, however, may be pretty confidently asserted to be very near 28° 30′, which is the mean of many measurements on four crystals. Seen in a microscope fitted with an excellent eyepiece goniometer, planes of polarization in the crystals are evidently parallel and perpendicular to the planes 1 0 0, 0 0 1; but whether a plane of polarization bisects the acute angle of the wedge, *i. e.* is parallel to 0 1 0 or to 1 0 0, or whether 1 0 0 is equally inclined to the planes forming the wedge—in short, whether the crystal is oblique or prismatic, it is very difficult to determine. The mineral frequently presents itself, moreover, in what appear to be twinned forms; but the angles between the planes 1 0 0 in the two individuals are not sufficiently concordant, as measured on different crystals, to justify a speculation on the symbols of a twin face.

Several analyses of Waringtonite concur in establishing its formula as $3Cu''H'_2O_2+Cu''SO_4+H'_2O$, as is seen by the following numbers:—

		Percentage as calculated.	Average found.
4 equivs. copper	=126·72	= 53·99	54·48
4 equivs. oxygen.....	= 32·	= 13·63	(calc. 13·756)
1 equiv. sulphuric anhydride	= 40·	= 17·04	16·73
4 equivs. water	= 36·	= 15·34	14·64
	234·72	=100·00	99·606

It also contains traces of lime, magnesia, and iron, and appears to be generally mixed with a small proportion of another mineral, which is probably Brochantite, as Brochantite occurs in distinct crystals on some of the specimens of Waringtonite.

Its specific gravity is 3·39 to 3·47.

Its hardness is 3 to 3·5, being harder than calcite, and about equal in hardness to celestine.

The entire difference of its crystallographic habit, the absence of the striation and marked prismatic forms so characteristic of Brochantite, its habitually paler colour, lower specific gravity (in Bro-

chantite G = 3·87 to 3·9), and hardness sufficiently distinguish it from that mineral. The mountain-green streak offers an available means of contrasting Waringtonite and Brochantite with Atacamite, the streak of which is of a characteristic apple-green.

M. Pisani has published analyses of the two above-described minerals. In the former (possibly from having driven off part of the water in the preliminary desiccation of the mineral) he has found less water than I consider it really to contain, and he has consequently given to Langite the formula of Waringtonite.

The green mineral which he has analyzed and described as Brochantite seems, from his analysis, to have contained a slight admixture of the ferruginous matrix, and also differs from mine in the estimate of the water.

I confined my preliminary desiccation to a careful treatment of the bruised mineral with dried and warm blotting-paper, as many hydrated minerals of this class yield up part of their water when long exposed to a perfectly dry air, or to a temperature of 100° C.

"Preliminary Note on the Radiation from a Revolving Disk." By Balfour Stewart, M.A., F.R.S., and P. G. Tait, M.A.

The authors having been led by perfectly distinct trains of reasoning to identical views bearing on the dissipation of energy, have had preliminary experiments made on the increase of radiation from a wooden disk on account of its velocity of rotation, both in the open air and *in vacuo*.

These experiments were made with a very delicate thermo-electric pile and galvanometer. In the experiments in the open air the disk was of wood; its diameter was 9 inches, and it was made to rotate with a velocity somewhat less than 100 revolutions in one second.

A sensible effect was produced upon the indicating galvanometer when the disk was made to rotate, and this effect appeared to be due to radiation, and not to currents of air impinging against the pile. In amount it was found to be nearly the same as if the disk had increased in temperature 0°·75 Fahr.

In the experiments *in vacuo* the diameter of the wooden disk was over 12 inches; its velocity of rotation was about 100 revolutions in one second, and the pile was nearer it than when in air. Under these circumstances, with a vacuum of 0·6 in., an effect apparently due to radiant heat was obtained, amounting to nearly the same as if the disk had increased in temperature 1°·5 Fahr.

Bearing in mind the increased diameter of the disk, the effect is probably equivalent to that obtained in air, and these preliminary experiments would tend to show that when a wooden disk is made to revolve rapidly at the surface of the earth, its radiation is increased to an extent depending on the velocity; and it would appear that this effect is not materially less in a vacuum of 0·6 in. than in the open air.

The authors intend to work out this and allied questions experimentally, and hope, if successful, to communicate the result to this Society.

PM 29 [47] (1865)

April 6.—Major-General Sabine, President, in the Chair.

The following communication was read:—

"Report on the New Unit of Electrical Resistance proposed and issued by the Committee on Electrical Standards appointed in 1861 by the British Association." By Fleeming Jenkin, Esq.

Sir Humphry Davy, in 1821*, published his researches proving a difference in the conducting-power of metals and the decrease of that power as their temperature rose. This quality of metals was examined by Snow Harris, Cumming, and E. Becquerel, whose table of conducting-powers, compiled by the aid of his differential galvanometer, and published in 1826†, is still frequently quoted, and is indeed remarkable as the result of experiments made before the publication by Ohm, in 1827‡, of the true mathematical theory of the galvanic circuit.

The idea of resistance as the property of a conductor was introduced by Ohm, who conceived the force of the battery overcoming the resistance of the conductors and producing the current as a result. Sir Humphry Davy, on the contrary, and other writers of his time, conceived the voltaic battery rather as continually reproducing a charge, somewhat analogous to that of a Leyden jar, which was discharged so soon as a conductor allowed the fluid to pass. The idea of resistance is the necessary corollary of the conception of a force doing some kind of work§, whereas the idea of conducting-power is the result of an obvious analogy when electricity is conceived as a fluid, or two fluids, allowed to pass in different quantities through different wires from pole to pole. When submitted to measurement, the qualities of conducting-power and resistance are naturally expressed by reciprocal numbers, and the terms are used in this sense in the early writings of Lenz (1833)‖, who, with Fechner¶, and Pouillet**, established the truth of Ohm's theory shortly after the year 1830.

The conception of a unit of resistance is implicitly contained in the very expression of Ohm's law; but the earlier writers seem to have contented themselves with reducing by calculation the resistance of all parts of a heterogeneous circuit into a given length of some given part of that circuit, so as to form an imaginary homogeneous conductor, the idea of which lies at the basis of Ohm's reasoning. These writers, therefore, generally speak of the resistance as the "reduced length" of the conductor, a term still much used in France (*vide* Daguin, Jamin, Becquerel, De la Rive, and others). The

* Phil. Trans. 1821, vol. cxi. p. 425.

† Ann. de Chim. et de Phys. vol. xxxii. 2nd series, p. 420.

‡ Die galvanische Kette, mathematisch bearbeitet, 1827; also Taylor's Scientific Memoirs, vol. ii. p. 401.

§ The writer does not mean by this that electrical and mechanical resistance are truly analogous, or that a current truly represents work.

‖ Pogg. Ann. vol. xxxiv. p. 418.

¶ Maasbestimmungen, etc. 1 vol. 4to. Leipzic, 1831.

** Elémens de Physique, p. 210, 5th edition; and Comptes Rendus, vol. iv. p. 267.

next step would naturally be, when comparing different circuits, to reduce all resistances into a length of some one standard wire, though this wire might not form part of all or of any of the circuits, and then to treat the unit length of that standard wire as a unit of resistance. Accordingly we find Lenz (in 1838*) stating that 1 foot of No. 11 copper wire is his unit of resistance, and that it is 19·9 times as great as the unit he used in 1833†, which was a certain constant part of the old circuit. In the earlier paper the resistances are treated as lengths, in the later as so many "units."

Lenz appears to have chosen his unit at random, and apparently without the wish to impose that unit upon others. A further advance is seen when Professor Wheatstone, in his well-known paper of 1843‡, proposes 1 foot of copper wire, weighing 100 grains, not only as a unit, but as a standard of resistance, chosen with reference to the standard weight and length used in this country. To Professor Wheatstone also appears due the credit of constructing (in 1840) the first instruments by which definite multiples of the resistance-unit chosen might be added or subtracted at will from the circuit‡. He was closely followed by Poggendorff§ and Jacobi‖, the description of whose apparatus, indeed, precedes that of the Rheostat and Resistance-coils, although the writer understands that they acknowledge having cognizance of those inventions. Resistance-coils, as the means of adding, not given lengths, but given graduated resistances to any circuit, are now as necessary to the electrician as the balance to the chemist.

In 1846 Hankel¶ used as unit of resistance a certain iron wire; in 1847 I. B. Cooke** speaks of a length of wire of such section and conducting-power as is best fitted for a standard of resistance. Buff†† and Horsford‡‡ in the same year reduce the resistance of their experiments to lengths of a given German-silver wire, and as a further definition they give its value as compared with pure silver. To avoid the growing inconvenience of this multiplicity of standards, Jacobi §§ (in 1848) sent to Poggendorff and others a certain copper wire, since well known as Jacobi's standard, desiring that they would take copies of it, so that all their results might be expressed in one measure. He pointed out, with great justice, that mere definition of the standard used, as a given length and weight of wire, was insufficient, and that good copies of a standard, even if chosen at random, would be preferable to the reproduction in one laboratory of a standard prepared and kept in another. The present Committee fully indorse this view, although the definition of standards based on weights and dimensions of given materials has since then gained greatly in precision.

Until about the year 1850 measurements of resistance were confined, with few exceptions, to the laboratory; but about that time

* Pogg. Ann. vol. xlv. p. 105. † Pogg. Ann. vol. xxiv. p. 418.
‡ Phil. Trans. 1843. vol. cxxxiii. p. 303. § Pogg. Ann. vol. lii. p. 511.
‖ Pogg. Ann. vol. lii. p. 526; vol. liv. p. 347. ¶ Pogg. Ann. vol. lxix. p. 255.
** Phil. Mag. New Series, vol. xxx. p. 385. †† Pogg. Ann. vol. lxxiii. p. 497.
‡‡ Pogg. Ann. vol. lxx. p. 238, and Silliman's Journ. vol. v. p. 36.
§§ Comptes Rendus, 1851. vol. xxxiii. p. 277.

underground telegraphic wires were introduced, and were shortly followed by submarine cables, in the examination and manufacture of which the practical engineer soon found the benefit of a knowledge of electrical laws. Thus in 1847 the officers of the Electric and International Telegraph Company used resistance-coils made by Mr. W. F. Cooke, apparently multiples of Wheatstone's original standard, which was nearly equal to the No. 16 wire of commerce; and Mr. C. F. Varley* states that, even at that date, he used a rough mode of "distance testing." In 1850, Lieut. Werner Siemens† published two methods for determining, by experiments made at distant stations, the position of "a fault"—that is to say, a connexion between the earth and the conducting-wire of the line at some point between the stations. In one of these plans a resistance equal to that of the battery is used, and the addition of resistances is also suggested; and Sir Charles Bright, in a Patent dated 1852‡, gives an account of a plan for determining the position of a fault by the direct use of resistance-coils. Since that time new methods of testing for faults and of examining the quality of materials employed, and the condition of the line, have been continually invented, almost all turning, more or less, on the measurement of resistance; greater accuracy has been continually demanded in the adjustment of coils and other testing-apparatus, until we have now reached a point where we look back with surprise at the rough and ready means by which the great discoveries were made on which all our work is founded.

The first effect of the commercial use of resistance was to turn the "feet" of the laboratory into "miles" of telegraph wire. Thus we find employed as units, in England the mile of No. 16 copper wire§, in Germany the German mile of No. 8 iron wire, and in France the kilometre of iron wire of 4 millimetres diameter. Several other units were from time to time proposed by Langsdorf‖, Jacobi¶, Marié-Davy**, Weber††, W. Thomson‡‡, and others, with a gradually increasing perception of the points of chief importance in a standard; but none of these were generally accepted as the one recognized measure in any country. To remedy the continually increasing evils arising from the discrepancies invariably found between different sets of coils, Dr. Werner Siemens (in 1860§§) constructed standards, taking as unit the resistance of a column of chemically pure mercury 1 metre long, having a section equal to 1 millimetre square, and maintained at the temperature of 0° Centigrade‖‖.

* Letter to writer, 1865.

† Pogg. Ann. vol. lxxix. p. 481. ‡ Patent No. 14,331, dated Oct. 21, 1852.

§ A size much used in underground conductors, and equal in resistance to about double the length of the common No. 8 iron wire employed in aërial lines.

‖ Liebig's Ann. vol. lxxxv. p. 155. ¶ Pogg. Ann. vol. lxxviii. p. 173.

** Ann. Chim. et Phys. 3rd series, vol. ix. p. 410.

†† Pogg. Ann. vol. lxxxii. p. 337.

‡‡ Phil. Mag. Dec. 1851. 4th ser. vol. ii p. 551. §§ Pogg. Ann. vol. cx. p. 1.

‖‖ Dr. Siemens, while retaining his definition, has altered the value of his standard about 2 per cent. since the first issue; and it is doubtful whether even the present standard represents the definition truly: his experiments were made by weight; and in reducing the results to simple measurements of length he has used a specific gravity for mercury of 13·557 instead of 13·596 as given by Regnault, 13·595 by H. Kopp, and 13·594 by Balfour Stewart.

Dr. Siemens supposed that this standard could be reproduced without much difficulty where copies could not be directly obtained. Mercury had been proposed before as a fitting material for a standard by Marié-Davy and De la Rive; but Dr. Siemens merits especial recognition, as the coils and apparatus he issued have been made with great care, and have materially helped in introducing strict accuracy*.

The question had reached this point when (in 1861) the British Association, at the suggestion of Professor W. Thomson, appointed a Committee to determine the best standard of electrical resistance. This Committee, aided by a grant from the Royal Society, has now issued a new standard, the subject of the present paper.

The writer has hitherto described those units only which are founded on a more or less arbitrary size and weight of some more or less suitable material; but measurements of resistance can be conceived and carried out entirely without reference to the special qualities of any material whatever. In 1849 Kirchhoff† had already effected a measurement of this kind; but it is to W. Weber‡ that we owe the first distinct proposal (in 1851) of a definite system of electrical measurements, according to which resistance would be measured in terms of an absolute velocity. This system of measures he called absolute electromagnetic measure, in analogy with Gauss's nomenclature of absolute magnetic measure. The Committee have decided that Weber's proposal is far preferable to the use of any unit of the kind previously described. Setting aside the difficulties in the way of their reproduction, which are by no means contemptible, arbitrary material standards, whether of mercury, gold, silver, platinum, or any other material, would be heterogeneous isolated units without any natural connexion with any other physical units. The unit proposed by Weber, on the other hand, forms part of a symmetrical natural system, including both the fundamental units of length, time, and mass, and the derived electrical units of current quantity and electromotive force. Moreover it has been shown by Professor W. Thomson§, who accepted and extended Weber's proposal immediately on its appearance, that the unit of absolute work, the connecting link between all physical forces, forms part of the same system, and may be used as the basis of the definition of the absolute electromagnetic units.

The full grounds of the choice of the Committee could only be explained by a needless repetition of the arguments given in the reports already made to the British Association. It will be sufficient here to state that, in the absolute electromagnetic system, the following equations exist between the mechanical and electrical units:—

$$W = C^2 R t, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

where W is the work done in the time t by the current C conveyed

* Many of the different units described above were represented by resistance-coils in the International Exhibition of 1862: *vide* Jury Report, Class XIII. p. 83, where their relative values are given: *vide* also Appendix A. to present paper.

† Pogg. Ann. vol. lxxvi. p. 412. ‡ Ibid. vol. lxxxii. p. 337.

§ Phil. Mag. Dec. 1851, 4th series, vol. ii. p. 551.

through a conductor of the resistance R. This equation expresses Joule and Thomson's law.

$$C=\frac{E}{R}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

where E is the electromotive force. This equation expresses Ohm's law.

$$Q=Ct, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

expressing a relation first proved by Faraday, where Q is the quantity of electricity conveyed or neutralized by the current in the time t. Finally, the whole system is rendered determinate by the condition that the unit length of the unit current must produce the unit force on the unit pole (Gauss) at the unit distance. If it is preferred to omit the conception of magnetism, this last statement is exactly equivalent to saying that the unit current conducted round two circles of unit area in vertical planes at right angles to each other, one circuit being at a great distance D above the other, will cause a couple to act between the circuits of a magnitude equal to the reciprocal of the cube of the distance D. This last relation expresses the proposal made by Weber for connecting the electric and magnetic measure. These four relations serve to define the four magnitudes R, C, Q, and E, without reference to any but the fundamental units of time, space, and mass; and when reduced to these fundamental units, it will be found that the measurement of R involves simply a velocity, *i. e.* the quotient of a length by a time. It is for this reason that the absolute measure of resistance is styled $\frac{\text{metre}}{\text{second}}$ or $\frac{\text{foot}}{\text{second}}$, precisely as the common non-absolute unit of work involving the product of a weight into a length is styled kilogrammetre or foot-pound. The Committee have chosen as fundamental units the second of time, the metre, and the mass of the Paris gramme. The metrical rather than the British system of units was selected, in the hope that the new unit might so find better acceptance abroad, and with the feeling that while there is a possibility that we may accept foreign measures, there is no chance that the Continent will adopt ours. The unit of force is taken as the force capable of producing in one second a velocity of one metre per second in the mass of a Paris gramme, and the unit of work as that which would be done by the above force acting through one metre of space. These points are very fully explained in the British Association Report for 1863, and in the Appendix C to that Report by Professor J. Clerk Maxwell and the writer *.

The magnitude of the $\frac{\text{metre}}{\text{second}}$ is far too small to be practically convenient, and the Committee have therefore, while adopting the system, chosen as their standard a decimal multiple 10^{10} times as great as Weber's unit $\left(\text{the}\ \frac{\text{millimetre}}{\text{second}}\right)$, or 10^7 times as great as the $\frac{\text{metre}}{\text{second}}$. This magnitude is not very different from Siemens's mercury unit, which has been found convenient in practice. It is about the twenty-fifth part of the mile of No. 16 impure copper wire used as

* [See pp. 409, 507 of the present volume of this Magazine.]

a standard by the Electric and International Company, and about once and a half Jacobi's unit*.

It was found necessary to undertake entirely fresh experiments in order to determine the actual value of the abstract standard, and to express the same in a material standard which might form the basis of sets of resistance-coils to be used in the usual manner. These experiments, made during two years with two distinct sets of apparatus by Professor J. C. Maxwell and the writer, according to a plan devised by Professor W. Thomson, are fully described in the Reports to the British Association for 1863 and 1864.

The results of the two series of experiments made in the two years agree within 0·2 per cent., and they show that the new standard does not probably differ from true absolute measure by 0·1 per cent†. It is not far from the mean of a somewhat widely differing series of determinations by Weber.

In order to avoid the inconvenience of a fluctuating standard, it is proposed that the new standard shall not be called "absolute measure," or described as so many $\frac{\text{metre}}{\text{seconds}}$, but that it shall receive a distinctive name, such as the B. A. unit, or, as Mr. Latimer Clark suggests, the "Ohmad," so that, if hereafter improved methods of determination in absolute measure are discovered or better experiments made, the standard need not be changed, but a small coefficient of correction applied in those cases in which it is necessary to convert the B. A. measure into absolute measure. Every unit in popular use has a distinctive name; we say feet or grains, not units of length or units of weight; and it is in this way only that ambiguity can be avoided. There are many absolute measures, according as the foot and grain, the millimetre and milligramme, the metre and gramme, &c. are used as the basis of the system. Another chance of error arises from the possibility of a mistake in the decimal multiple used as standard. For all these reasons, as well as for convenience of expression, the writer would be glad if Mr. Clark's proposal were adopted and the unit called an Ohmad.

Experiments have been made for the Committee by Dr. Matthiessen, to determine how far the permanency of material standards may be relied on, and under what conditions wires unaltered in dimension, in chemical composition, or in temperature change their resistance. Dr. Matthiessen has established that in some metals a partial annealing, diminishing their resistance, does take place, apparently due to age only. Other metals exhibit no alteration of this kind; and no permanent change due to the passage of voltaic currents has been detected in any wires of any metal—a conclusion contrary to a belief which has very generally prevailed.

The standard obtained has been expressed in platinum, in a gold-

* This last number may be 30 per cent. wrong, as the writer has never been in possession of an authenticated Jacobi standard, and has only arrived at a rough idea of its value by a series of published values which afford an indirect comparison.

† *Vide* Appendix B.

silver alloy, in a platinum-silver alloy, in a platinum-iridium alloy, and in mercury. Two equal standards have been prepared in each metal; so that should time or accident cause a change in one or more, this change will be detected by reference to the others. The experiments and considerations which have led to the choice of the above materials are fully given in the Report to the British Association for 1864. The standards of solid metals are wires of from 0·5 millim. to 0·8 millim. diameter, and varying from one to two metres in length, insulated with white silk wound round a long hollow bobbin, and then saturated with solid paraffin. The long hollow form chosen allows the coils rapidly to assume the temperature of any surrounding medium, and they can be plunged, without injury, into a bath of water at the temperature at which they correctly express the standard. The mercury standards consist of two glass tubes about three-quarters of a metre in length. All these standards are equal to one another at some temperature stated on each coil, and lying between 14°·5 and 16°·5 C. None of them, when correct, differ more than 0·03 per cent. from their value at 15°·5 C.

Serious errors have occasionally been introduced into observations by resistance at connexions between different parts of a voltaic circuit, as perfect metallic contact at these points is often prevented by oxide or dirt of some kind. Professor Thomson's method of inserting resistances in the Wheatstone balance (differential measurer) has been adopted for the standards, but in the use of the copies which have been issued it has been thought that sufficient accuracy would be attained by the use of amalgamated mercury connexions.

In the standards themselves permanence is the one paramount quality to be aimed at; but in copies for practical use a material which changes little in resistance with change of temperature is very desirable, as otherwise much time is lost in waiting till coils have cooled after the passage of a current; moreover large corrections have otherwise to be employed when the coils are used at various temperatures; and these temperatures are frequently not known with perfect accuracy. German silver, a suitable material in this respect, and much used hitherto, has been found to alter in resistance, in some cases, without any known cause but the lapse of time, since the change has been observed where the wires were carefully protected against mechanical or chemical injury. A platinum-silver alloy has been preferred by the Committee to German silver for the copies which have been made of the standard. These have been adjusted by Dr. Matthiessen so as to be correct at some temperature not differing more than 1° from 15°·5 C. The resistance of platinum-silver changes about 0·031 per cent. for each degree Centigrade within the limits of 5° above and below this temperature; this change is even less than that of German silver. The new material seems also likely to be very permanent, as it is little affected by annealing. The form of the copies is the same as that of the standard, with the exception of the terminals, which are simple copper rods ending in an amalgamated surface. Twenty copies have been distributed gratis, and notices issued that others can be procured from the Committee

for £2 10*s*. The Committee also propose to verify, at a small charge, any coils made by opticians, as is done for thermometers and barometers at Kew.

Dr. Matthiessen reports, with reference to the question of reproduction, that given weights and dimensions of several pure metals might be employed for this purpose *if absolute care were taken*. The reproduction, in this manner, of the mercury unit, as defined by Dr. Siemens, differs from the standards issued by him in 1864 about 8·2 per thousand if the same specific gravity of mercury be used for both observations*. Each observer uses for his final value the mean of several extremely accordant results. It is therefore to be hoped that the standard will never have to be reproduced by this or any similar method. On the other hand, four distinct observers, with four different apparatus, using four different pairs of standards issued respectively by Dr. Siemens and the Committee, give the B. A. unit as respectively equal to 1·0456, 1·0455, 1·0456, and 1·0457 of Siemens's 1864 unit. It is certain that two resistances can be compared with an accuracy of one part in one hundred thousand—an accuracy wholly unattainable in any reproduction by weights and measures of a given body, or by fresh reference to experiments on the absolute resistance. The above four comparisons, two of which were made by practical engineers, show how far the present practice and requirements differ from those of twenty and even ten years ago, when, although the change of resistance due to change of temperature was known, it was not thought necessary to specify the temperature at which the copper or silver standard used was correct. The difficulty of reproducing a standard by simple reference to a pure metal, further shows the unsatisfactory nature of that system in which the conducting-power of substances is measured by comparison with that of some other body, such as silver or mercury. Dr. Matthiessen has frequently pointed out the discrepancies thus produced, although he has himself followed the same system pending the final selection of a unit of resistance. It is hoped that for the future this quality of materials will always be expressed as a specific resistance or specific conducting-power referred to the unit of mass or the unit of volume, and measured in terms of the standard unit resistance, that the words conducting-power will invariably be used to signify the reciprocal of resistance, and that the vague terms good and bad conductor or insulator will be replaced, in all writings aiming at scientific accuracy, by those exact measurements which can now be made with far greater ease than equally accurate measurements of length.

There is every reason to believe that the new standard will be gladly accepted throughout Great Britain and the colonies. Indeed the only obstacle to its introduction arises from the difficulty of explaining to inquirers what the unit is. The writer has been so much perplexed by this simple question, finding himself unable to answer it without entering at large on the subject of electrical mea-

* If Dr. Matthiessen uses the sp. gr. of 13·596, as given by Regnault, the difference from Dr. Siemens's standard is 5 per thousand

surement, that he has been led to devise the following definitions, in which none but already established measures are referred to.

The resistance of the absolute $\frac{\text{metre}}{\text{second}}$ is such that the current generated in a circuit of that resistance by the electromotive force due to a straight bar 1 metre long moving across a magnetic field of unit intensity* perpendicularly to the lines of force and to its own direction with a velocity of 1 metre per second, would, if doing no other work or equivalent of work, develope in that circuit in one second of time a total amount of heat equivalent to one absolute unit of work—or sufficient heat, according to Dr. Joule's experiments, to heat 0·0002405 gramme of water at its maximum density 1° Centigrade

The new standard issued is as close an approximation as could be obtained by the Committee to a resistance ten million times as great as the absolute $\frac{\text{metre}}{\text{second}}$. The straight bar moving as described above in a magnetic field of unit intensity, would require to move with a velocity of ten millions of metres per second to produce an electromotive force which would generate in a circuit of the resistance of the new standard the same current as would be produced in the circuit of one $\frac{\text{metre}}{\text{second}}$ resistance by the electromotive force due to the motion of the bar at a velocity of one metre per second. The velocity required to produce this particular current† being in each case proportional to the resistance of the circuit, may be used to measure that resistance, and the resistance of the B. A. unit may therefore be said to be ten millions of metres per second, or $10^7 \frac{\text{metres}}{\text{second}}$.

It is feared that these statements are still too complex to fulfil the purpose of popular definitions, but they may serve at least to show how a real velocity may be used to measure a resistance by using the velocity with which, under certain circumstances, part of a circuit must be made to move in order to induce a given current in a circuit of the resistance to be measured. That current in the absolute system is the unit current, and the work done by that unit current in the unit of time is equal to the resistance of the circuit, as results from the first equation stated above.

Those who from this slight sketch may desire to know more of the subject will find full information in the Reports of the Committee to the British Association in 1862, 1863, and 1864. The Committee continue to act with the view of establishing and issuing the correlative units of current, electromotive force, quantity, and capacity, the standard apparatus for which will, it is proposed, be deposited at Kew along with the ten standards of resistance already constructed with the funds voted by the Royal Society.

* Gauss's definition.

† This current is the unit current, and, if doing no other work or equivalent of work, would develope, in a circuit of the resistance of the B. A unit, heat equivalent to ten millions of units of work, or enough to raise the temperature of 2405 grammes of water at its maximum density 1° Centigrade.

Appendix B.

The following Table shows the degree of concordance obtained in the separate experiments used to determine the unit. The determinations were made by observing the deflections of a certain magnet when a coil revolved at a given speed, first in one direction, and then in the opposite direction. The first column shows the speed in each experiment; the second shows the value of the B. A. unit in terms of $10^7 \frac{\text{metres}}{\text{second}}$, as calculated from the single experiments. A difference constantly in one direction may be observed in the values obtained when the coil revolved different ways. This difference depended on a slight bias of the suspending thread in one direction. The third column shows the value of the B. A. unit calculated from the pair of experiments. The fourth shows the error of the pair from the mean value finally adopted. In the final mean adopted, the 1864 determination was allowed five times the weight allowed to that of 1863.

1. Time of 100 revolutions of coil, in seconds.	2. Value of B. A. unit in terms of $10^7 \frac{\text{metres}}{\text{second}}$, as calculated from each experiment.	3. Value from mean of each pair of experiments.	4. Percentage error of pair of observations from mean value.
17·54	1·0121	0·9978	−0·22
17·58	0·9836		
77·62	1·0468	1·0040	+0·40
76·17	0·9613		
53·97	0·9985	0·9992	−0·08
54·53	0·9998		
41·76	0·9915	0·9925	−0·75
41·79	0·9936		
54·07	0·9961	0·9924	−0·76
53·78	0·9886		
17·697	0·9878	1·0007	+0·07
17·783	1·0136		
17·81	0·9952	1·0063	+0·63
17·78	1·0174		
17·01	1·0191	1·0043	+0·43
16·89	0·9895		
21·35	1·0034	1·0022	+0·22
21·38	1·0011		
21·362	0·9968	1·0040	+0·40
21·643	1·0096		
11·247	1·0424	0·9981	−0·19
16·737	0·9707		

Probable error of R (1864)..... =0·1 per cent.
Probable error of R (1863)..... =0·24 ,,
Difference in two values 1864 and 1863=0·16 ,,
Probable error of two experiments .... =0·08 ,,

LXVI. *Intelligence and Miscellaneous Articles.*

AIR-PUMP CONSTRUCTED ON A NEW PRINCIPLE. BY M. DELEUIL.

THE machine is intended for industrial purposes, as it is only purposed to try to obtain, in a relatively short time, a vacuum of 18 millims. of mercury for the size of vessels commonly worked with, and of 8 millims. for the usual sizes of the laboratory. The principle on which I have gone has much analogy with that which guided M. Isoar, ten or twelve years ago, in his superheated steam-engine, which consisted in using steam at high pressures, acting on pistons of small section working with great velocity, and not rubbing against the sides of the cylinder. I imagined that if, in making a vacuum, I caused a metallic piston to move in a cylinder perfectly; and only leaving between it and the cylinder the thickness of a sheet of letter-paper, the fluid could not pass from one side to the other of the piston, provided that its length was equal to at least twice its diameter, and it was provided with grooves 8 or 10 millims. apart. Experiment has shown that with such a piston, without any great velocity, a vacuum of from 8 to 18 millims. may be attained, according to the capacities. The fluid itself serves as packing for the piston. I thus, at the same time, destroy the resistance due to the friction of the piston in the barrel and the stopping up of the valves (by suppressing the oil used to lubricate the pump), as well as the wear and tear of the cylinder. This machine is double-acting, and can be used as compression-pump up to the limit of two atmospheres, as it can pump gas from a reservoir, and compress it in another without appreciable loss of gas.—*Comptes Rendus*, March 20, 1865.

METEOR AND METEORITES OF ORGUEIL.

On the evening of the 14th of May, 1864, a very bright fireball was seen in France throughout the whole region from Paris to the Pyrenees. Loud detonations were heard in the neighbourhood of Montauban, and a large number of stones came down near the villages of Orgueil and Nohic. The passage of the meteor was witnessed by a large number of intelligent observers, since it occurred early in the evening. Numerous accounts of its appearance have been published in the *Comptes Rendus*.

This fall of meteorites is of peculiar interest. While we have over a hundred large fireballs and detonating meteors whose paths through the atmosphere have been computed with more or less precision, there are only four or five of them from which stones have been known to come. Of these four or five, only one, the Weston meteor, has been so well observed that we can speak with confidence of its path.

The published accounts show that the Orgueil meteor was first seen at an altitude greater than 55 miles, that it exploded at an altitude of about 20 miles, and that it was descending in a line inclined at the least 20° or 25° to the horizon. The velocity must have been not less than 15 or 20 miles per second. This example affords the strongest proof that the stone-producing meteors and the detonating meteors are phenomena not essentially unlike.—Silliman's *American Journal for* March 1865.

PHENOMENON IN THE INDUCTION-SPARK. BY E. FERNET.

The disengagement of heat which the induction-spark produces in the air exerts upon the path of this spark an influence which seems to be shown by the following experiment.

Two small straight brass rods about 2 decimetres in length, each upon an insulating support, are placed almost vertically and parallel to each other at a distance of a few centimetres; they are then moved somewhat apart above, so that they form below a very acute angle. They are then both united with the ends of the induction-coil of a Ruhmkorff's apparatus. The sparks which pass at each oscillation of the commutator appear first, as is natural, between the two nearest points of the bars—that is, at the bottom. But they soon leave this and appear at a higher part, until they reach the highest, when this discharge suddenly ceases. The spark now passes below, and the same series of phenomena is repeated. The duration of the impression has, moreover, the effect that not merely one line of light is seen, but several are seen close together—a sort of ladder with very brilliant rounds in the dark, which slowly and regularly ascends between the vertical bars, breaks off, and then again begins from below without ever exhibiting the inverse direction.

These results appear to be explained by the heating in the discharge. The passage of each spark produces in the air a considerable increase of temperature, the air expands, ascends, and thus the upper layer, though longer, offers less resistance, in consequence of which the second spark passes here. The passage of the second acts just in the same way upon the third, and so forth, until the discharge takes place on the uppermost points. The air continues to ascend; but the spark betakes itself where the layer of air is shortest, that is, to the lowest point.

This explanation is supported by a change of the experiment. For if the bars are placed in a horizontal, and not a vertical plane, still somewhat divergent, no displacement is observed, but the spark always passes between the nearest points. This is also the case if the bars are vertical but converge above. Even in the first position of all, the ascent of the spark can be suddenly suppressed, if a current of air from above is directed upon it.—*Comptes Rendus*, vol. lix. p. 1005.

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SUPPLEMENT TO VOL. XXIX. FOURTH SERIES.

LXVII. *On a new Determination of the Lengths of Waves of Light, and on a Method of determining, by Optics, the Translatory Motion of the Solar System.* By A. J. ÅNGSTRÖM*.

[With a Plate.]

I.

IN the Note on Fraunhofer's lines which I had the honour of communicating to the Royal Academy in October 1861, I spoke of my intention of revising the lengths of luminous waves, as determined by Fraunhofer†, and of extending these determinations to all the remarkable lines of the spectrum, in order with their help to obtain the wave-lengths for the metal-spectra.

The weather last summer was, on the whole, scarcely favourable to such experiments on the solar spectrum, nor are these experiments by any means complete. Nevertheless, since my measurements of the principal lines of Fraunhofer are sufficiently numerous and self-accordant to secure my results from any essential change, I have deemed it of some interest to examine whether, and to what extent, these new determinations agree with those obtained by Fraunhofer himself—the more so because no new measurements on the wave-lengths of light have, to my knowledge, been made since Fraunhofer closed his wonderful investigations.

I employed in my experiments an optical theodolite constructed by Pistor and Martins in Berlin, and a glass grating made by the optician Nobert in Barth. The theodolite was provided with two telescopes, the second of which served as a sight-indicator (*Sehzeichen*). In reading off, two microscopes were used, and one division of the micrometer corresponded to an angle of $2''{\cdot}1$.

The eyepiece is also provided with a micrometer arrangement: the screw-head is divided into 100 parts; and when the telescope

* From Poggendorff's *Annalen*, vol. cxxiii. p. 489; to which journal the paper was communicated by the Author after its publication in the *Oefverigt af K. Vet. Akad. Förh.* 1863, No. 2.

† Poggendorff's *Annalen*, vol. cxvii. p. 290.

is adjusted on an infinitely distant object, every scale-division corresponds to $1''{\cdot}308$.

The glass grating prepared by Nobert is particularly well constructed. In a space 9·0155 Par. lines broad, there are 4501 lines drawn by a diamond. Errors of division, as tested by Nobert with a microscope which magnified 800 times, lie below 0·00002 of a Par. line.

The breadth, as given by Nobert, was obtained by comparison with a standard prepared by the mechanician Baumann of Berlin, and which was a copy of the one made by the same artist for Bessel.

As a proof of the excellence of this glass grating, I may state that Fraunhofer's lines can be seen therewith in the third and fourth spectrum, and that in distinctness and richness of detail these lines far exceed those which are obtained by the refraction of light through a flint-glass prism.

During the observations the grating was always placed perpendicularly to the incident rays. This was accomplished, *first*, by always giving to the unscratched side of the grating a position such that the image of the heliostat-aperture reflected by it coincided with the aperture itself; *secondly*, by adjusting on the heliostat-aperture the moveable telescope used in the observations; and *thirdly*, by fixing the axis of the second telescope so as to coincide with the prolongation of the optic axis of the first.

The scratched side of the glass grating was always turned from the incident light and towards the moveable telescope, being placed in the middle over the rotation-axis of the instrument.

The observations were calculated according to the known formula

$$e \sin \Theta = m\lambda,$$

where e, or the distance between two scratches on the grating, had, according to the above remark, the value

$$e = 0{\cdot}000166954 \text{ of a Par. inch},$$

λ denotes the required wave-length, Θ the observed angle, and m the order of the spectrum.

As the values of λ thus obtained have reference to air, they must be dependent upon its temperature and barometric pressure; I have consequently always noted these two elements, although under ordinary circumstances their influence on the measurements was found to be inappreciable. The changes in the temperature of the grating itself exercise a somewhat more important action; nevertheless since, at the time the observations were made (September and commencement of October), the temperature of the room only oscillated between 13° and 18° C., I have likewise omitted this correction.

That no appreciable errors can have thereby arisen in the mean values thus obtained—values which may be regarded as true for 15° C. and the mean barometric pressure—is readily seen on calculating the magnitudes of these corrections.

Assuming the refraction-coefficient of air to be

$$n = 1{\cdot}000294,$$

$\frac{n-1}{d}$ to be a constant magnitude, independent of temperature and pressure, and the value of e, moreover, to hold for 15° C., w obtain the following corrected value :—

$$\log \lambda = \log \frac{e \sin \Theta}{m} - 0{\cdot}36\,(t_e^\circ - 15^\circ) + 0{\cdot}09(t_n^\circ - 15^\circ) - 0{\cdot}04(\mathrm{H} - 0^{\mathrm{m}}{\cdot}76),$$

whence we conclude that the correction for $\log \lambda$ amounts to

$$+ 0{\cdot}45\,(t^\circ - 15^\circ) - 0{\cdot}14\,(\mathrm{H} - 0^{\mathrm{m}}{\cdot}76),$$

expressed in units of the fifth decimal place.

Accordingly a change of 2 degrees in temperature produces a change of $2''$ in the value of the angle Θ, if Θ be assumed equal to 25°; this error is comparable with the error of adjustment itself. For smaller values of Θ the error will of course be smaller.

The angle Θ is also subject to a correction dependent upon the absolute motion of the instrument in the direction of the path of the incident ray; this correction, however, is almost inappreciable for the observations upon which the numerical values in the following Table are founded.

The wave-lengths are, like those of Fraunhofer, expressed in units whose magnitude is equal to 0·00000001 of a Par. inch.

TABLE I.—Wave-lengths, in $\frac{1}{100{,}000{,}000}$ths of a Paris inch.

B.	Spectrum.	C.	Spectrum.	D.	Spectrum.	E.	Spectrum.	*b*.	Spectrum.	F.	Spectrum.	G.	Spectrum.	H.	Spectrum.	H_1.	Spectrum.
2539·91	1	2426·50	1	2178·69	3	1948·25	1	1916·51	1	1797·38	1	1592·32	2	1467·19	1	1454·88	4
2539·54	2	2426·28	2	2178·53	3	1948·21	3	1916·64	3	1797·37	3	1592·53	1	1467·58	4	1453·39	3
2539·76	3	2426·23	2	2178·62	2	1948·24	2	1916·46	2	1797·21	3	1592·22	2	1467·32	3	1453·74	2
		2426·33	3	2178·64	1	1948·20	1	1916·53	4	1797·27	2	1592·16	2	1466·66	2	1453·89	1
		2426·25	1	2178·57	4	1948·25	3	1916·56	1	1797·05	1	1592·50	2	1467·12	1		
		2426·27	2	2178·61	4	1948·24	3	1916·49	4	1797·20	3	1592·32	2	1467·34	4		
				2178·56	2	1948·23	3	1916·43	4	1797·11	2	.....		1466·98	3		
				2178·48	4	1948·32	4	1916·47	4	1797·55	4						
2539·73		2426·29		2178·59		1948·24		1916·50		1797·27		1592·34		1467·18		1453·98	

The difference of the wave-lengths corresponding to the two D lines, as measured in the third and in the fourth spectrum, amounts to 2·226,—that between the wave-lengths corresponding to the two E lines being only 0·395, as measured in the third spectrum.

Fraunhofer has given two different series of values for the wave-lengths of light. The first series was obtained by measurements with wire gratings, and it is upon this that Cauchy founded his calculations in the *Mémoire sur la Dispersion*. It contains the following numerical values (β):—

B.	C.	D.	E.	F.	G.	H.
2541,	2425,	2175,	1943,	1789,	1585,	1451.

Comparing these values with the corresponding ones in the foregoing Table, which I will call the series (α), the following differences ($\alpha-\beta$) are obtained:—

$$-1{\cdot}3,\ +1{\cdot}3,\ +3{\cdot}6,\ +5{\cdot}2,\ +8{\cdot}3,\ +7{\cdot}4,\ +16{\cdot}2.$$

The differences increase, as will be seen, towards the violet end of the spectrum, and are there very considerable. This arises from the difficulty, when using gratings so coarse as those employed by Fraunhofer, of accurately distinguishing the dark lines at the violet end of the spectrum.

The best of all the gratings employed by Fraunhofer is, without doubt, that which he denoted as No. 4, and with which he observed the line E even in the thirteenth spectrum. This grating gives, in general, values which agree better with my own. For the lines C, D, and E the agreement is nearly perfect. The grating in question gave, in fact, the values

B.	C.	D.	E.	F.	G.	H.
2542,	2426,	2178,	1947,	1794,	1586,	1457.

I conclude from this that the disagreement between the series (α) and (β) must arise principally from errors of observation, which, with the wire gratings used by Fraunhofer, were unavoidable.

The other series of values of wave-lengths given by Fraunhofer is of a somewhat later date. It will be found in Gilbert's *Annalen der Physik*, vol. lxxiv., as well as in Herschel's 'Optics,' Schwerd's *Beugungs-Erscheinungen*, and other works. This series, on account of its exactitude, appears to have been held by Fraunhofer in greater esteem than the older ones.

It contains the following values (γ):—

C.	D.	E.	F.	G.	H.
2422,	2175,	1945,	1794,	1587,	1464;

and gives, when compared with the series (α), the differences ($\alpha-\gamma$): $+4{\cdot}3,\ +3{\cdot}6,\ +3{\cdot}2,\ +3{\cdot}3,\ +5{\cdot}4,\ +3{\cdot}1.$

The values of the wave-lengths contained in the series (γ) depend on measurements of the first interference-spectrum of a *glass grating* which was considerably finer than the one I employed. According to Fraunhofer's statement, in fact,

$$e = 0{\cdot}0001223 \text{ of a Par. inch.}$$

Since, however, the number of marks in this grating of Fraunhofer's amounted only to 3601, the breadth reduces itself to

$$5{\cdot}2833 \text{ Par. lines;}$$

and consequently it must have been considerably less luminous than that of Nobert. In another respect, too, Fraunhofer's grating, although an excellent one, appears to me to have been inferior to that of Nobert; for the line B could not be measured even in the first spectrum, and the lines from C to G were not visible in any of the spectra beyond the second.

Nevertheless, since almost all the differences ($\alpha - \gamma$) have the same value, a constant error appears to be indicated, either in my measurements or in those of Fraunhofer. That an error of this character cannot have affected the value of Θ in *my measurements*, is evident from the fact that the value of this angle was obtained from mutually agreeing observations on four different spectra. The introduction of such an error into Fraunhofer's measurements is equally inadmissible, since on calculating the wave-lengths of the lines from C to G (which Fraunhofer also observed in the second interference-spectrum, though he did not introduce them into his calculation), the following mutually according values are obtained from the two spectra:—

	C.	D.	E.	F.	G.
First spectrum .	2422·00,	2174·58,	1944·81,	1793·98,	1586·89;
Second spectrum.	2421·54,	2174·36,	1944·63,	1793·92,	1588·07.

It is only for the line G that the difference is somewhat greater.

The reason of the differences ($\alpha - \gamma$), therefore, must arise from an erroneous determination of the value of e; which latter may have been caused either by a wrong enumeration of the lines in one of the two gratings, or by an incorrect estimation of their breadth. In order to make the two values of the wave-lengths for the line D agree, in the series (α) and (γ), by altering the value of e, the breadth of Nobert's grating would have to be *diminished* by

$$0{\cdot}0123 \text{ of a Par. line} = 0{\cdot}001025 \text{ of a Par. inch,}$$

or the number of lines in the grating *increased* by 6.

The same object would be attained by *increasing* the breadth

of Fraunhofer's grating by

0·00061 of a Par. inch,

or by *diminishing* the number of lines by 5.

That the second decimal is wrong in the above breadth (=9·0155 lines) of Nobert's grating is not probable; far more so is the supposition of an error of about half this magnitude in the estimation of the breadth of Fraunhofer's grating, especially since the microscope, forty years ago, had not reached its present high degree of perfection. Fraunhofer, moreover, was compelled to *strengthen the extreme lines* of his grating, in order to see them more distinctly when measuring, a circumstance which may possibly have affected the positions of these two lines.

Besides the fact that my measurements agree with the results which Fraunhofer obtained by means of the grating No. 4, there is another reason in favour of the assumption that the differences $(\alpha-\gamma)$ arise from an incorrect value of e in Fraunhofer's glass grating. For the above-cited memoir of Fraunhofer's contains measurements made with another glass grating for which e had the considerably greater value of

0·0005919 of a Paris inch.

Fraunhofer made no use of these measurements, probably because this grating proved to be far less perfect, the spectra on one side of the axis being twice as intense as those on the other. On calculating these measurements, however, we obtain the following values corresponding to the lines from D to G:—

D.	E.	F.	G.	Spectrum.
2177·25	1947·21	....	....	5
2177·48	1947·18	1796·10	....	4
2177·64	1947·23	1796·09	1590·90	3
2176·80	1946·63	1795·99	1591·07	2
2177·55	1947·25	1796·39	1590·16	1
2177·34	1947·10	1796·14	1590·71	

These values, compared with the series (α), indicate a constant difference; here, however, the differences amount only to

1·25, 1·14, 1·13, 1·63,

or to about one-third of those last given.

Now, since this last grating was nearly five times as coarse as the former, and probably also broader, it must have been easier to determine accurately its corresponding e. This circumstance

tends to increase the probability of the existence of an error in the value of e corresponding to the finer grating.

The values of the wave-lengths obtained by means of Nobert's grating, therefore, appear to me to merit a greater confidence than that which Fraunhofer's can justly claim.

II.

As already stated at the commencement of this paper, I have not limited my measurements to the principal lines of Fraunhofer. I have measured, with the circle, the angle Θ for all the stronger lines at a distance from each other of from 10′ to 20′, and determined with the eyepiece-micrometer the positions of the remaining intermediate lines. The measurements, moreover, were repeated in the second, third, and fourth spectra, in order to verify their exactitude.

The following Table contains some of these results, those wave-lengths alone being given which correspond to the strongest and most prominent lines of the solar spectrum. Most of these lines belong to iron or to lime, and have consequently a double interest, since they present themselves also in the gas-spectra of these substances. In order to give the reader a visible image of the position and breadth of these lines in the solar spectrum, I have added a figure (Plate III. fig. 1), which correctly shows their respective positions as presented by a prism of sulphide of carbon having an angle of 60°. An arc of 2′ corresponds in the figure to a length of one millimetre.

Table II.—Wave-lengths, in hundred millionths ($=\frac{1}{10^8}$) of a Paris inch.

Line.	Wave-length.	Spectra in which corresponding lines are observed.	Remarks.
A	2812		
B	2539·7		
C	2426·29		
α	2312·2	Earth's atmosphere ...	Strong line.
	2287·3		
	2279·6		
	2276·8		
	2269·4	Iron and calcium	Group of strong lines.
	2267·7		
	2262·1		
	2255·1		
D	2179·70		
	2177·48		
1	2076·1	Iron.	
2	2071·3	Iron	Two groups of lines.
3	2069·7		
4	2068·3		
5	2065·4		

TABLE II. (*continued*).

Line.	Wave-length.	Spectra in which corresponding lines are observed.	Remarks.
6	2060·1	Iron.	
7	2016·9	,,	
8	2013·6	,,	
	2013·1	,,	
9	2007·3	,,	
10	2005·3	,,	
11	1998·4	,,	
	1997·9	,,	
12	1985·8	,, ...	Strong line.
	1985·3		
	1984·2	,, ...	Weak.
	1983·5		
13	1974·2	,, ...	Double line, like E.
14	1969·6	,,	
15	1968·1	,,	
16	1965·3	,,	
17	1953·2	,,	
E	1948·44	Iron and calcium.	
	1948·04		
	1946·8	,, ,, ...	Double line, like E.
	1934·6	Iron.	
	1936·4	,,	
	1919·6	,, ...	Double line, like E.
b	1916·50	Magnesium.	
b_1	1912·39	,,	
b_{11}	1911·10	Iron and magnesium.	
	1910·49	Iron.	
	1903·4	,,	
c	1832·70	,,	
	1819·1	,, ...	Iron when weakly incandescent gave but one of these lines; when strongly incandescent, however, a third was visible.
	1818·4		
	1808·3	,, ...	Double.
	1801·1	,,	
F	1797·27	Hydrogen.	
f	1632·2	Iron.	
	1628·5	,,	
	1620·4	,,	
	1604·3	Hydrogen.	
	1598·8	Iron.	
G	1592·34	,,	
	1579·1	,,	
	1574·7	,,	
	1571·2	,,	
g	1562·4	Calcium	Double line.
	1532·0	Iron	Double line; several weak lines were also visible between *g* and *h*.
h	1515·9	Unknown.	Very strong line.
	1505·3	Iron	Strong line.
	1502·0	,, ...	,,
	1495·2	,, ...	,,
	1480·4	Unknown	,,
H	1467·2	Calcium.	
H	1454·0	,,	

III.

In a lecture given on October 6, 1860, to the Royal Scientific Society of Upsala, I explained a method of determining the motion of the solar system by observations on the interference-bands of a glass grating. I then showed that if we assume the propagation of the undiffracted rays, passing through the openings of the grating, to be uninfluenced by the motion of the instrument, the same must be true of the formation of the interference-bands on both sides; consequently, also, that when a telescope is used in the observations the customary aberration must ensue, and be proportional to the ratio between the motion of the telescope, in a direction perpendicular to its axis, and the velocity of light along this axis.

Hence, the velocity of light being taken as the unit, if h be the velocity of the instrument in the direction of the incident light, then for an angle Θ, under which, *e. g.*, the D line in an interference-spectrum is observed, the velocity of the telescope perpendicular to this direction will be

$$h \sin \Theta,$$

which accordingly must be the expression for the aberration.

If the angle Θ were observed for two positions of the instrument in which the velocities in the path of the incident rays were h and h', we should then have

$$\Delta\Theta = (h - h') \sin \Theta, \quad \ldots\ldots \quad (1)$$

or, since 2Θ is the angle immediately given by observation,

$$\Delta . 2\Theta = 2(h - h') \sin \Theta.$$

Putting $h(=-h')$ equal to the velocity of the earth in its orbit, this equation gives

$$\Delta . 2\Theta = 81''{\cdot}6 \sin \Theta;$$

and since, for the double line D in the fourth spectrum,

$$2\Theta = 62^\circ\ 55'\ 44''{\cdot}2,$$

we deduce

$$\Delta . 2\Delta = 42''{\cdot}6,$$

a magnitude capable of being readily observed.

Two questions have here to be answered by observation. The one has reference to the actual existence of the phenomenon, and may be most readily answered by applying the method to the known orbital motion of the earth; the other has reference to the employment of the method, when proved to be accurate, to the determination of the translatory motion of the solar system.

The experiments hitherto made cannot in any respect be con-

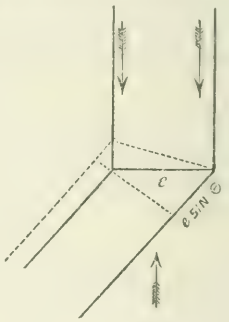
sidered as quite decisive. Last midsummer the weather was unfavourable to my observations, and at the end of October the latter were not sufficiently numerous to furnish an answer even to the first of the above questions.

I should not in fact have alluded to the subject had not M. Babinet, in the Academy of Sciences, proposed a method of determining the translatory motion of the solar system identical with the one which, two years ago, I submitted to the Royal Scientific Society of Upsala.

A small difference exists, however, in our calculations. I had assumed the motion of the grating to have no influence on the angle Θ, whereas Babinet introduces, on this account, the correction

$$h(1-\cos\Theta)\tan\Theta.$$

The truth of this formula may in fact be readily established by help of the adjoining figure, in which $e\sin\Theta$ denotes the distance traversed by light during the time that the grating describes the distance $-he\sin\Theta$ in a direction contrary to that of the incident rays. The difference of path for the two interfering waves will consequently, through the motion of the grating, be diminished by



$$he\,(1-\cos\Theta)\sin\Theta,$$

a magnitude which, when equated to

$$-e\cos\Theta\,d\Theta,$$

gives

$$d\Theta=-h(1-\cos\Theta)\tan\Theta.$$

The value of $d\Theta$ will, of course, be positive when the instrument moves in the same direction as the light.

The expression thus obtained, added to the one in the formula (1), gives for the total variation of the angle Θ the value

$$\Delta\Theta=(h-h')\tan\Theta\,;$$

and if, moreover, $h=-h'=20''{\cdot}4$, and

$$2\Theta=62^\circ\,55'\,41'',$$

then will

$$\Delta 2\Theta=49''{\cdot}8.$$

Hence in the special case under consideration, the variation of the angle 2Θ is increased by $7''{\cdot}2$ in consequence of the motion of the grating.

The observations on which the numerical values of Table I.

are based were all (with a few exceptions) made at or near mid-day. On this account I thought that the corrections due to the motion of the instrument might be neglected in calculating the results, since in the final mean such corrections must, for the most part, disappear.

In proof of the accuracy of the theory here established, I will give a few of the observations made last year at the commencement of October. They have reference to the double line D in the fourth interference spectrum. The light was always incident from *south* to *north*. The second telescope and the grating were readjusted every day.

Table III.

Time of observation, in 1862.			$2\Theta_4=\phi$.	Remarks.
	h		° ′ ″	
Oct. 5	11·4	A.M.	62 55 38	Mean of three observations.
	3·58	P.M.	62 55 53	
	5	P.M.	62 56 7	Mean of six observations.
Oct. 9, 10	3·74	P.M.	62 56 0	Mean of six observations.
Oct. 11	9·5	A.M.	62 55 51	Mean of two observations.
	1	P.M.	62 55 58	
	3·75	P.M.	62 56 7	

From the mean value of the wave-lengths corresponding to the line D given in Table I. we deduce

$$2\Theta_4=62^\circ\ 55'\ 41''{\cdot}2=\phi_0;$$

and since this value must be very nearly free from any error due to the motion of the instrument, it ought to agree with that furnished by the observations in Table III., after applying to the latter the corrections due to the motion of the instrument.

If X be the velocity of the solar system in a direction determined by the coordinates of the equator,

$$D=34^\circ{\cdot}5 \text{ and } A=259^\circ{\cdot}8,$$

the magnitude of the motion of the instrument *from north to south*, due to the motion of the solar system, will be

$$X\cos b=X[\cos D\sin P\cos(A-*)-\sin D\cos P],$$

where P denotes the altitude of the pole, and $*$ the sidereal time of the observation.

For Upsala, therefore, we shall have the formula

$$X[0{\cdot}713\cos(259^\circ{\cdot}8-*)-0{\cdot}284].$$

The velocity of the instrument, in the above direction, due to

the earth's *annual motion* is equal to

$$h \cos b_1 = h \{ \cos D_1 \sin P \sin [\odot - *] - \sin D_1 \cos P \},$$

where

$$- \sin D_1 = \sin 23^\circ\ 38' \cos \odot.$$

In this formula $\odot$ denotes the right ascension of the sun, P and $*$ the same magnitudes as before, and $h = 20''{\cdot}4$ the velocity of the earth expressed by the angle it subtends at the centre of a circle whose radius is the velocity of light. The total correction of the angle ϕ, therefore, will be

$$\Delta\phi = 24''{\cdot}9 [\cos b_1 + n \cos b],$$

since

$$X = nh \text{ and } 40''{\cdot}8 \tan \Theta = 24''{\cdot}9.$$

If by means of this last formula, and under different assumptions for the value of n, we calculate the correction for each angle ϕ in Table III., and afterwards add these corrections to their respective angles, the resulting values of $\phi + \Delta\phi$, subtracted from the assumed true value of $2\Theta_4$, that is to say, from

$$\phi_0 = 62^\circ\ 55'\ 41'',$$

will give the following:

TABLE IV.

$\phi_0 - \phi$.	$\phi_0 - (\phi + \Delta\phi)$.			
	$n = 0$.	$n = \frac{1}{3}$.	$n = \frac{1}{2}$.	$n = 1$.
$+\ 3''$	$+\ 3''$	$+\ 4''$	$+\ 4''$	$+\ 7''$
-11	$+\ 9$	$+\ 5$	$+\ 3$	$-\ 2$
-26	$-\ 3$	$-\ 6$	$-\ 7$	-13
-19	$+\ 2$	$-\ 1$	$-\ 3$	$-\ 9$
-10	-17	-14	-12	$-\ 8$
-18	$-\ 7$	-10	-10	-12
-26	$-\ 5$	-10	-10	-16

The sums of the squares of the differences are respectively 2267, 462, 419, 427, 719.

So far as we can conclude from the above observations, *the influence of the earth's annual motion* appears to be verified; that of the motion of the solar system is less perceptible. Nevertheless it is obvious that if we were to assume that motion to be zero, or to be equal to that of the earth in its orbit, the agreement between the observations would be worse than under the assumption that the magnitude of the motion in question is

somewhat more than one-third of that of the earth. Between this result, and what we already know of the motion of the solar system through astronomy, there is no great divergence.

I hope during the present year, however, to be able to continue my spectrum-experiments, and to have a better opportunity of determining, numerically, the magnitude of the motion of the solar system. In the present paper my object has merely been to show the possibility of solving, optically, this interesting problem in physical astronomy.

LXVIII. *On the Intersections of a Pencil of four Lines by a Pencil of two Lines.* *By* Professor Cayley, *F.R.S.**

Plücker has considered ("Analytisch-geometrische Aphorismen," *Crelle*, vol. xi. (1834) pp. 26–32) the theory of the eight points which are the intersections of a pencil of four lines by any two lines, or say the intersections of a pencil of *four* lines by a pencil of *two* lines: viz., the eight points may be connected two together by twelve new lines; the twelve lines meet two together in forty-two new points; and of these, six lie on a line through the centre of the two-line pencil, twelve lie four together on three lines through the centre of the four-line pencil, and twenty-four lie two together on twelve lines, also through the centre of the four-line pencil.

The first and third of these theorems, viz. (1) that the six points lie on a line through the centre of the two-line pencil, and (3) that the twenty-four points lie two together on twelve lines through the centre of the four-line pencil, belong to the more simple theory of the intersections of a pencil of *three* lines by a pencil of *two* lines; the second theorem, viz. (2) the twelve points lie four together on three lines through the centre of the four-line pencil, is the only one which properly belongs to the theory of the intersections of a pencil of *four* lines by a pencil of *two* lines. The theorem in question (proved analytically by Plücker) may be proved geometrically by means of two fundamental theorems of the geometry of position: these are the theorem of two triangles in perspective, and Pascal's theorem for a line-pair. I proceed to show how this is.

Consider a pencil of two lines meeting a pencil of four lines in the eight points (a, b, c, d), (a', b', c', d'); so that the two lines are $abcd$, $a'b'c'd'$ meeting suppose in Q; and the four lines are aa', bb', cc', dd' meeting suppose in P; then the twelve points are

* Communicated by the Author.

$a'd \,.\, c'b$, $ad' \,.\, cb'$, $a'c \,.\, d'b$, $ac' \,.\, db'$ lying in a line through P,
$a'b \,.\, d'c$, $ab' \,.\, dc'$, $a'd \,.\, b'c$, $ad' \,.\, bc'$,, ,,
$a'c \,.\, b'd$, $ac' \,.\, bd'$, $a'b \,.\, c'd$, $ab' \,.\, cd'$,, ,,

where the combinations are most easily formed as follows; viz., for the first four points starting from the arrangement $\begin{matrix} a & c \\ d & b \end{matrix}$ (or any other arrangement having the diagonals $ab \,.\, cd$), and thence writing down the four expressions

$$\begin{matrix} a'c' & a\,c & a'\,c & a\,c' \\ d\,b' & d'\,b' & d'\,b' & d\,b' \end{matrix}$$

we read off from these the symbols of the four points; and the like for the other two sets of four points.

Now, considering the points (a, b, c) and (a', b', c'), the points $ab' \,.\, a'b$, $ac' \,.\, a'c$, $bc' \,.\, b'c$ lie in a line through Q; and similarly the points $ab' \,.\, a'b$, $ad' \,.\, a'd$, $bd' \,.\, b'd$ lie in a line through Q; which lines, inasmuch as they each contain the points Q and $ab' \,.\, a'b$, must be one and the same line; considering the combinations (b, c, d), (b', c', d'), the line in question also passes through $cd' \,.\, c'd$; that is, the six points $ab' \,.\, a'b$, $ac' \,.\, a'c$, $ad' \,.\, a'd$, $bc' \,.\, b'c$, $bd' \,.\, b'd$, $cd' \,.\, c'd$ lie in a line through Q, which is in fact the before-mentioned first theorem. Hence the points $ab' \,.\, a'b$ and $cd' \,.\, c'd$ lie in a line through Q; or, calling these points M and N respectively, the triangles Maa', Mbb', Ncc', Ndd' are in perspective. Hence, considering the two triangles Maa', Ndd' (or, if we please, the complementary set Mbb', Ncc'), the corresponding sides are

Ma, Nd meeting in $ab' \,.\, dc'$,
Ma', Nd' ,, $a'b \,.\, d'c$,
aa' , dd' ,, P ;

that is, the points $ab' \,.\, dc'$, $a'b \,.\, d'c$ lie in a line through P.

Similarly $ad' \,.\, a'd$ and $bc' \,.\, b'c$ lie in a line through Q; or, calling these points H, I respectively, the triangles Haa', Hdd', Ibb', Icc' are in perspective; and considering the combination Hdd', Ibb' (or, if we please, the complementary set Haa', Icc'), the corresponding sides are

Ha, Ib meeting in $ad' \,.\, bc'$,
Ha', Ib' ,, $a'd \,.\, cb'$,
aa', bb' ,, P ;

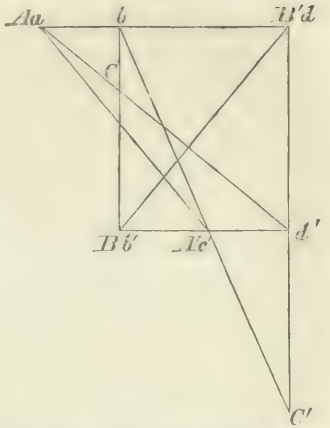
that is, the points $a'd \,.\, c'b$, $ad' \,.\, cb'$ lie in a line through P.

It remains to be shown that the two lines through P, viz. the line containing $ab' \,.\, dc'$ and $a'b \,.\, d'c$, and the line containing $ad' \,.\, bc'$ and $a'd \,.\, cb'$, are one and the same line. This will be

the case if, for instance, $ab' \,.\, dc'$ and $ad' \,.\, bc'$ also lie in a line through P.

We have the points (a, b, d) in a line, and the points (b', c', d') in a line; the points a, d, b', c' are also called A, B′, B, A′ respectively; ad', bb' meet in C, bc', dd' in C′; hence, considering the hexagon $ad'db'bc'$, the lines

ad', $b'b$ meet in C ,
$d'd$, bc' ,, C′ ,
db', ca' ,, AA′ . BB′;



and hence these three points lie in a line; or, what is the same thing, the lines AA′, BB′, and CC′ meet in a point, that is, the triangles ABC, A′B′C′ are in perspective; the corresponding sides are

AB, A′B′, that is, ab', $c'd$ meeting in $ab' \,.\, c'd$,
BC, B′C′ ,, $b'b$, $d'd$,, P ,
CA, C′A′ ,, ad', bc' ,, $ad' \,.\, bc'$;

and these three points lie in a line; that is, the points $ab' \,.\, dc'$ and $ad' \,.\, bc'$ lie in a line through P. Hence the line through $ab' \,.\, dc'$ and $a'b \,.\, d'c$ and the line through $ad' \,.\, bc'$ and $a'd \,.\, cb'$ are one and the same line; that is,

the points $ab' \,.\, dc'$, $a'b \,.\, d'c$, $ad' \,.\, bc'$, $a'd \,.\, b'c$ lie in a line through P.

This proves the existence of one of the lines through P; and that of the other two lines follows from the symmetry of the figure; it thus appears that the twelve points lie four together on three lines through P.

Cambridge, April 11, 1865.

LXIX. *On Hemiopsy, or Half-vision.*

By Sir David Brewster, *K.H., F.R.S.**

THE affection of Half-vision, or Half-blindness as it has been called, was first distinctly described by Dr. Wollaston, in a paper "On Semidecussation of the Optic Nerves," published in the Philosophical Transactions for 1824. "It is now more

* From the Transactions of the Royal Society of Edinburgh, vol. xxiv. part 1. Communicated by the Author.

than twenty years," he says, "since I was first affected with this peculiar state of vision, in consequence of violent exercise I had taken for two or three hours before. I suddenly found that I could see but half the face of a man whom I met, and it was the same with every object I looked at. In attempting to read the name Johnson over a door, I saw only son, the commencement of the name being wholly obliterated from my view. In this instance, the loss of sight was towards my left, and was the same, whether I looked with my right eye or my left. This blindness was not so complete as to amount to absolute blackness, but was a shaded darkness, without definite outline. The complaint lasted only about a quarter of an hour." In 1822, Dr. Wollaston had another attack of hemiopsy, with this difference, that the blindness was to the right of the centre of vision; and he has referred to three other cases among his friends; but in these the affection was accompanied with headache and indigestion.

In republishing Dr. Wollaston's paper in the *Annales de Chimie et de Physique**, M. Arago says that he knows *four* cases of hemiopsy, and that he himself had experienced three attacks of it, followed by headache above the right eye.

In the 'Cyclopædia of Practical Surgery,' published in 1841, Mr. Tyrrell describes Hemiopsy as "Functional amaurosis from general disturbance." He informs us that "he has experienced this form of amaurosis several times," and that he has been consulted by several fellow-sufferers of both sexes. In all these cases the affection was attended with severe headache, giddiness, and gastric irritation, sometimes preceding, and sometimes following the attack.

In the accounts which have been given of these different cases of hemiopsy, no attempt has been made to ascertain the optical condition of the eye when it is said to be half-blind, or to determine the locality and immediate cause of the complaint. Dr. Wollaston describes the blindness as a shaded darkness without definite outline. M. Arago says nothing about darkness; and the insensibility of the retina, of which he speaks, must mean its insensibility to visual, and not to luminous impressions. Mr. Tyrrell, on the other hand, simply states that the obscurity takes place in different portions of the retina, and varies in its extent at different times.

Having myself experienced several attacks of hemiopsy, I have been enabled to ascertain the optical condition of the retina when under its influence, and to determine the extent of the affection, and its immediate cause.

In reading the different cases of hemiopsy, we are led to infer

* 1824, vol. xxvii. p. 109.

that there is vision in one-half of the retina, and blindness in the other. But this is not the case. The blindness, or insensibility to distinct impressions, exists chiefly in a small portion of the retina to the right or left hand of the *foramen centrale*, and extends itself irregularly to other parts of the retina on the same side, in the neighbourhood of which the vision is uninjured. In some cases the upper half of the object is invisible, the part of the retina paralyzed being a little below the *foramen centrale*. On some occasions, in absolute darkness, when a faint glow of light was produced by some uniform pressure upon the whole of the retina, I have observed a great number of black spots, corresponding to parts of the retina upon which no pressure was exerted.

In the case of ordinary hemiopsy, as observed by myself, there is neither darkness nor obscurity, the portion of the paper from which the letters disappear being as bright as those upon which they are seen. Now this is a remarkable condition of the retina. While it is sensible to luminous impressions, it is insensible to the lines and shades of the pictures which it receives of external objects; or, in other words, the retina is in certain parts of it in such a state that the light which falls upon it is irradiated, or passes into the dark lines or shades of the pictures upon it, and obliterates them. This irradiation exists to a small degree, even when the vision is perfect at the *foramen centrale*, and it may be produced artificially in a sound eye, on parts of the retina remote from the foramen, and as completely, though temporarily, as in hemiopsy. In order to prove this, we have only to look obliquely at a narrow strip of paper placed upon a green cloth—that is, to fix the eye upon a point a little distant from the strip of paper. After a short time the strip of paper will disappear partially or wholly, and the space which it occupied will be green, or the colour of the ground upon which it is laid*.

This temporary insensibility of the retina in the part of it covered by the picture of the strip of paper, or its inability to maintain constant vision of it, can arise only from its being paralyzed by the continued action of light,—an effect not likely to be produced, and never observed, in the ordinary use of the eye.

The insensibility of the retina in cases of hemiopsy, and the consequent irradiation of the light into the space occupied with the letters, or the objects which disappear, though a phenomenon of the same kind as that which takes place in oblique vision, has yet a very different origin. The parts which are in these cases affected extend irregularly from the *foramen centrale*

* Letters on Natural Magic. Lett. II. p. 13.

to the margin of the retina, as if they were related to the distribution of its blood-vessels and hence it was probable that the paralysis of the corresponding parts of the retina was produced by their pressure. This opinion might have long remained merely a reasonable explanation of hemiopsy, had not a phenomenon presented itself to me which places it beyond a doubt. When I had a rather severe attack, which never took place unless I had been reading for a long time the small print of the 'Times' newspaper, and which was never accompanied either with headache or gastric irritation, I went accidentally into a dark room, when I was surprised to observe that all the parts of the retina which were affected were slightly luminous, an effect invariably produced by pressure upon that membrane. If these views be correct, hemiopsy cannot be regarded as a case of amaurosis, or in any way connected, as has been supposed, with cerebral disturbance.

Dr. Wollaston endeavoured to explain the phenomena of hemiopsy, and the fact of single vision with two eyes, by what he calls the semidecussation of the optic nerves, a doctrine which Sir Isaac Newton had suggested, and employed to account for single vision*. A fibre of the right-hand side of the optic nerve is supposed to semidecussate or divide itself into two fibres, sending *one* to the right side of the right eye, and *another* to the right side of the left eye, while a fibre on the left-hand side of the optic nerve also semidecussates, sending *one* fibre to the left side of the left eye, and *another* to the left side of the right eye. Hence Sir Isaac Newton drew the conclusion, that an impression on each of the two half fibres would convey a single sensation to the brain; and hence Dr. Wollaston concluded that hemiopsy in one eye must be accompanied with hemiopsy in the other.

Ingenious as these explanations are, the anatomical facts by which alone they could be supported have not been established. Dr. Alison†, who has adopted the opinion of Newton, and reasoned upon it, admits that the anatomical evidence is still defective; and the late Mr. Twining‡ has adduced nine cases of disease in the optic nerves and thalami, which stand in direct opposition to the hypothesis of semidecussation. Dr. Mackenzie, too, adopting the same view of the subject as Mr. Twining, distinctly asserts that "the great mass of facts in Pathology and Experimental Anatomy, touching this question, go to prove that injuries and diseases affecting one side of the brain, instead of *hemiopsia* in both eyes, produce amaurosis only in the opposite eye."

* Optics, p. 320.

† Edinburgh Transactions, vol. xiii. p. 479.

‡ Trans. Med. Soc. Calcutta, vol. ii. p. 151; or Edinburgh Journal o Science, July 1828, vol. ix. p. 143.

The two great facts of hemiopsy in both eyes, and of what is called single vision with two eyes, do not require the hypothesis of semidecussation to explain them. If hemiopsy is produced by the distended blood-vessels of the retina, these vessels must be similarly distributed in each eye, and similarly affected by any change in the system; and consequently must produce the same effect upon each retina, and upon the same part of it.

In explaining single vision with two eyes, we have no occasion to appeal to double fibres in the optic nerves, or to corresponding points on the retina. There is, in reality, no such thing as single vision, that is, a single image seen by both eyes. With two sound eyes every object is seen double, and it appears single only when, by the law of visible position, the one image is placed above the other. But even in this case the object is seen double, by means of two dissimilar images of it which are not coincident. By shutting the right eye we lose sight of a part on the right side of the double image, which is seen only by the right eye; and by shutting the left eye we lose sight of a part on the left side of the double image, which is seen only by the left eye. If one eye gives a better picture than the other, the duplicity of the apparently single image is more easily seen. By shutting the good eye the imperfect picture is seen, and by shutting the bad eye we insulate the perfect picture. It is difficult to understand how optical writers and physiologists should have so long demanded a single sensation for the production of a single picture from the two pictures imprinted on the two retinas. If we had the hundred eyes of Argus, the production of an apparently single picture would have been the necessary result of the Law of Visible Position.

LXX. *On the Elementary Relations between Electrical Measurements. By* Professor J. Clerk Maxwell and Fleeming Jenkin, *Esq.*

[Concluded from p. 460.]

Part IV.—*Measurement of Electric Phenomena by Statical Effects.*

33. *Electrostatic Measure of Electric Quantity.*—By the application of a sufficient electromotive force between two parts of a conductor which does not form a circuit, it is possible to communicate to either part a *charge* of electricity which may be maintained in both parts, if properly insulated (14). With the ordinary electromotive forces due to induction or chemical action, and the ordinary size of insulated conductors, the charge of

electricity in electromagnetic measure is exceedingly small; but when the capacity of the conductor is great, as in the case of long submarine cables, the charge may be considerable. By making use of the electromotive force produced by the friction of unlike substances, the charge or electrification even of small bodies may be made to produce visible effects. The electricity in a charge is not essentially in motion, as is the case with the electricity in a current. In other words, a charge may be permanently maintained without the performance of work. Electricity in this condition is therefore frequently spoken of as statical electricity; and its effects, to distinguish them from those produced by currents, may be called statical effects. The peculiar properties of electrically charged bodies are these:—

1. When one body is charged positively (14), some other body or bodies must be charged negatively to the same extent.

2. Two bodies repel one another when both are charged positively, or both negatively, and attract when oppositely charged.

3. These forces are inversely proportional to the square of the distance of the attracting or repelling charges of electricity.

4. If a body electrified in any given invariable manner be placed in the neighbourhood of any number of electrified bodies, it will experience a force which is the resultant of the forces that would be separately exerted upon it by the different bodies if they were placed in succession in the positions which they actually occupy, without any alteration in their electrical conditions.

From these propositions it follows that, at a given distance, the force, f, with which two small electrified bodies repel one another is proportional to the product of the charges, q and q_1, upon them; but when the distance varies, this force, f, is inversely proportional to the square of the distance, d, between them. Hence

$$f = \frac{qq_1}{d^2}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

When q and q_1 are of dissimilar signs, f becomes negative; *i. e.* there is an attraction, and not a repulsion. This equation is incompatible with the electromagnetic definitions given in Part III., and, if it be allowed to be fundamental, gives a new definition of the unit quantity of electricity, as that quantity which, if placed at unit distance from another equal quantity of the same kind, repels it with unit force.

34. *Electrostatic System of Units.*—This new measurement of quantity forms the foundation of a distinct system or series of units, which may be called the electrostatic units; and mea-

surements in these units will in these pages be designated by the use of small letters: thus, as Q, C, &c., signified quantity, current, &c., in *electromagnetic* measure, so q, c, e, and r, &c., will represent the *electrostatic* measure of quantity, current, electromotive force, resistance, &c.

The relations between current and quantity, between work, current, and electromotive force, and between electromotive force, current, and resistance, remain unchanged by the change from the electromagnetic to the electrostatic system.

35. *Ratio between Electrostatic and Electromagnetic Measures of Quantity.*—Since the expression forming the second member of equation (17) represents a force the dimensions of which are $\frac{LM}{T^2}$, the dimensions of q are $\frac{L^{\frac{3}{2}}M^{\frac{1}{2}}}{T}$. The dimensions of the unit of electricity, Q, in the electromagnetic system are $L^{\frac{1}{2}}M^{\frac{1}{2}}$ (25). Hence, since in passing from the one system to the other we must employ the ratio $\frac{q}{Q}$, this ratio will be of the dimensions $\frac{L}{T}$; that is to say, the ratio $\frac{q}{Q}$ is a velocity. In the present treatise this velocity will be designated by the letter v.

The first estimate of the relation between quantity of electricity measured statically and the quantity transferred by a current in a given time was made by Faraday*. A careful experimental investigation by MM. Weber and Kohlrausch† not only confirms the conclusion that the two kinds of measurement are consistent, but shows that the velocity $v = \frac{q}{Q}$ is 310,740,000 metres per second—a velocity not differing from the estimated velocity of light more than the different determinations of the latter quantity differ from each other. v must always be a constant, real velocity in nature, and should be measured in terms of the system of fundamental units adopted in electrical measurements (3) and (55). A redetermination of v (46) will form part of the present Committee's business in 1863–64. It will be seen that, by definition, the quantity transmitted by an electromagnetic unit current in the unit time is equal to v electrostatic units of quantity.

36. *Electrostatic Measure of Currents.*—In any coherent system, a current is measured by the quantity of electricity which passes in the unit of time (15); if both current and quantity

* Experimental Researches, series iii. § 361, &c.

† *Abhandlungen der König. Sächsischen Ges.* vol. iii. (1857) p. 260; or Poggendorff's *Annalen*, vol. xcix. p. 10 (Aug. 1856).

are measured in electrostatic units, then

$$c=\frac{q}{t}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

The dimensions of c are therefore $\frac{L^{\frac{3}{2}}M^{\frac{1}{2}}}{T^2}$; and in order to reduce a current from electromagnetic to electrostatic measure, we must multiply C by v, or

$$c=v\mathrm{C}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

37. *Electrostatic Measure of Electromotive Force.*—The statical measure of an electromotive force is the work which would be done by electrical forces during the passage of a unit of electricity from one point to another. The only difference between this definition and the electromagnetic definition (16 and 27) consists in the change of the unit of electricity from the electromagnetic to the electrostatic.

Hence, if q units of electricity are transferred from one place to another, the electromotive force between those places being e, the work done during the transfer will be qe; but we found (27) that if E and Q be the electromagnetic measures of the same quantities, the work done would be expressed by QE; hence

$$qe=\mathrm{QE};$$

but (35)

$$q=v\mathrm{Q},$$

therefore

$$e=\frac{\mathrm{E}}{v}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (20)$$

Thus, to reduce electromotive force from electromagnetic to electrostatic measure, we must divide by v.

The dimensions of e are $\frac{L^{\frac{1}{2}}M^{\frac{1}{2}}}{T}$.

38. *Electrostatic Measure of Resistance.*—If an electromotive force, e, act on a conductor whose resistance in electrostatic measure is r, and produce a current, c, then by Ohm's law

$$r=\frac{e}{c}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (21)$$

Substituting for e and c their equivalents in electromagnetic measure (equations 19 and 20), we have

$$r=\frac{1}{v^2}\frac{\mathrm{E}}{\mathrm{C}}$$

but (eq. 7)

$$\mathrm{R}=\frac{\mathrm{E}}{\mathrm{C}},$$

and therefore

$$r = \frac{1}{v^2} \mathrm{R}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

To reduce a resistance measured in electromagnetic units to its electrostatic value, we must divide by v^2.

The dimensions of r are $\frac{\mathrm{T}}{\mathrm{L}}$, or the reciprocal of a velocity.

39. *Electric Resistance in Electrostatic Units is measured by the Reciprocal of an Absolute Velocity.*—We have seen from the last paragraph that the dimensions of r establish this proposition; but the following independent definition, due to Professor W. Thomson, assists the mind in receiving this conception as a necessary natural truth. Conceive a sphere of radius k, charged with a given quantity of electricity Q. The potential of the sphere, when at a distance from all other bodies, will be $\frac{\mathrm{Q}}{k}$ (40, 41, and 47). Let it now be discharged through a certain resistance, r. Then, if the sphere could collapse with such a velocity that its potential should remain constant (or, in other words, that the ratio of the quantity on the sphere to its radius should remain constant, during the discharge), the time occupied by its radius in shrinking the unit of length would measure the resistance of the discharging conductor in electrostatic measure, or the velocity with which its radius diminished would measure the conducting-power (50) of the discharging conductor. Thus the conducting-power of a few yards of silk in dry weather might be an inch per second, in damp weather a yard per second. The resistance of 1000 miles of pure copper wire, $\frac{1}{16}$ inch in diameter, would be about 0·00000141 of a second per metre, or its conducting-power one metre per 0·00000141 of a second, or 708980 metres per second.

40. *Electrostatic Measure of the Capacity of a Conductor.*—The electrostatic capacity of a conductor is equal to the quantity of electricity with which it can be charged by the unit electromotive force. This definition is identical with that given of capacity measured in electromagnetic units (26). Let s be the capacity of a conductor, q the electricity in it, and e the electromotive force charging it; then

$$q = se. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

From this equation we can see that the dimension of the quantity s is a length only. It will also be seen that

$$s = v^2 \mathrm{S}, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

where S is the electromagnetic measure of the capacity of the conductor with the electrostatic capacity s.

The capacity of a spherical conductor in an open space is, in electrostatic measure, equal to the radius of the sphere,—a fact demonstrable from the fundamental equation (17).

Experimentally to determine s, the capacity of the conductor in electrostatic measure, charge it with a quantity, q, of electricity, and measure in any unit its potential (47) or tension (49), e. Then bring it into electrical connexion with another conductor whose capacity, s_1, is known. Measure the potential, e_1, of s and s_1 after the charge is divided between them; then

$$q = se = (s + s_1)e_1,$$

and hence

$$s = \frac{e_1}{e - e_1} s_1. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

In this measurement we do not require to know e and e_1 in absolute measure, since the ratio of these two quantities only is required. We must, however, know the value of s_1; and hence we must begin either with a spherical conductor in a large open space, whose capacity is measured by its radius, or with some other form of absolute condenser alluded to in the following paragraph.

41. *Absolute Condenser. Practical Measurement of Quantity.*—As soon as the electromotive force of a source of electricity is known in electrostatic measure, the quantity which it will produce in the form of charge on simple forms is known by the laws of electrical distribution experimentally proved by Coulomb. Simple forms of this kind may be termed *absolute condensers*. A sphere in an open space is such a condenser, and the quantity it contains is se (equation 23). A more convenient form is a sphere of radius x, suspended in the centre of a hollow sphere, radius y, the latter being in communication with the earth. The capacity, s, of the internal sphere is then, by calculation,

$$s = \frac{xy}{y - x}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (26)$$

By a series of condensers of increasing capacity we may measure the capacity of any condenser, however large. The comparison is made by the method described above (40). Thus, the practical method of measuring quantity in electrostatic measure is first to determine the capacity of the conductor containing the charge, and then to multiply that capacity by the electromotive force producing the charge (43).

42. *Practical Measurement of Currents:*—The electrostatic value of currents can be obtained from equation (21) when e and r are known, or from equation (19) when v and C are known,

or by comparison with a succession of discharges of known quantities from an absolute condenser.

43. *Practical Measurement of Electromotive Force.*—The relations expressed by equations (17) and (23) show that in any given circumstances the force exerted between two bodies due to the effects of statical electricity will be proportional to the electromotive force or difference of potential (47) between them. This fact allows us to construct gauges of electromotive force, or instruments so arranged that a given electromotive force between two parts of the apparatus brings an index into a sighted position. In order that the gauge may serve to *measure* the electromotive force absolutely, it is necessary that two things should be known,—first, the distribution of the electricity over the two attracting or repelling masses (or, in other words, the capacity of each part); secondly, the absolute force exerted between them. For simple forms, the distribution, or capacity of each part, can be calculated from the fundamental principles (33); the force actually exerted can be weighed by a balance. By these means Professor W. Thomson* determined the electromotive force of a Daniell's cell to be 0·0021 in British electrostatic units, or 0·0002951 in metrical electrostatic units†. This proposition is equivalent to saying that two balls of a metre radius, at a distance d apart in a large open space, and in connexion with the opposite poles of a Daniell's cell, would attract one another with a force equal to $\frac{(0{\cdot}0002951)^2}{4\,d^2}$ absolute units, or $\frac{0{\cdot}000,000,00888}{d^2}$ gramme weight‡.

An apparatus by which such a measurement as the foregoing can be carried out is called an absolute electrometer. It will be observed that, although the definition of electromotive force is founded on the idea of work, its practical measurement is effected by observing a force, inasmuch as when this force exerted between two conductors of simple shape is known, the work which the passage of a unit of electricity between them would perform may be calculated by known laws.

44. *Comparison of Electromotive Forces by their Statical Effects.*—This comparison is simpler than the absolute measurement, inasmuch as it is not necessary, in comparing two forces, to know the absolute values of either. Instruments by which the comparison can be made are called electrometers. Their arrange-

* Paper read before the Royal Society, February 1860. *Vide* Proceedings of the Royal Society, vol. x. p. 319, and Phil. Mag. S. 4. vol. xx. (1860) p. 233.

† [Note added May 5, 1865.—In electromagnetic measure this would make the electromotive force of a Daniell's cell equal to about 91,700. Other observers have found a value of about 100,000 (metrical system, based on metre, gramme, and second).]

‡ This value was erroneously given in the original paper.

ment is of necessity such that the force exerted between two given parts of the instrument shall be proportional to the difference of potential between them. This force may be variable and measured by the torsion of a wire, as in Thomson's reflecting electrometer; or it may be constant, and the electromotive forces producing it may be compared by measuring the distance required in each case between the two electrified bodies to produce that constant force. The latter arrangement is adopted in Professor Thomson's portable electrometer, first exhibited at the present Meeting of the Association. The indications of a gauge or electrometer not in itself absolute may be reduced to absolute measurement by multiplication into a constant coefficient.

45. *Practical Measurement of Electric Resistance.*—The electrostatic resistance of a conductor of great resistance (such as gutta percha or india rubber) might be directly obtained in the following manner:—Let a body of known capacity, s (40), be charged to a given potential, P (47), and let it be gradually discharged through the conductor of great resistance, r. Let the time, t, be noted at the end of which the potential of the body has fallen to p. The rate of loss of electricity will then be $\frac{p}{sr}$. Hence $p = \mathrm{P}_\epsilon^{-\frac{t}{sr}}$ and $\frac{t}{sr} = \log_\epsilon \frac{\mathrm{P}}{p}$. Hence

$$r = \frac{t}{s \log_\epsilon \frac{\mathrm{P}}{p}}; \quad \ldots\ldots\ldots \quad (27)$$

from which equation r can be deduced if s, t, and the ratio $\frac{\mathrm{P}}{p}$ be known; t can be directly observed; s can be measured (40); and the ratio $\frac{\mathrm{P}}{p}$ can be measured by an electrometer (44) in constant connexion with the charged body. This ratio can also be measured by the relative discharges through a galvanometer, first, immediately after the body has been charged to the potential P, and again when, after having been recharged to the potential P, it has, after a time t, fallen to potential p. (This latter plan has long been practically used by Messrs. Siemens, although the results have not been expressed in absolute measure.)

Unfortunately, in those bodies, such as gutta percha and india rubber, the resistance of which is sufficiently great to make t a measurable number, the phenomenon of absorption due to continued electrification* so complicates the experiment as to

* *Vide* British Association Report, 1859, Trans. of Sec. p. 248, and Report of the Committee of Board of Trade on Submarine Cables, pp. 136 & 464.

render it practically unavailable for any exact determination. The apparent effect of absorption is to cause r, the resistance of the material, to be a quantity variable with the time t, and the laws of the variation are very imperfectly known.

46. *Experimental Determination of the Ratio, v, between Electromagnetic and Electrostatic Measures of Quantity.*—In order to obtain the value of v, it is necessary and sufficient that we should obtain a common electrostatic and electromagnetic measure of some one quantity, current, resistance, electromotive force, or capacity. There are thus five known methods by which the value can be obtained.

(1) By a common measure of quantity. Let a condenser of known capacity, s, be prepared (40). Let it be charged to a given potential P (47). Then the quantity in the condenser will be sP in electrostatic measure. The charge can next be measured by discharge through a galvanometer (25) in electromagnetic measure. The ratio between the two numbers will give the value of v. The only difficulty in this method consists in the measurement of the potential P entailing the measurement of an absolute force between two electrified bodies. This method was proposed and adopted by Weber*.

(2) By a comparison of the measure of electromotive force. The electromotive force produced by a battery, in electrostatic measure, can be directly weighed (43). Its electromotive force, in electromagnetic measure, can be obtained from the current it produces in a given resistance (28). The ratio of the two numbers will give the value of v. This method has been carried out by Professor W. Thomson, who was not, however, at the time in possession of the means of determining accurately either the absolute resistance of his circuit or the absolute value of the current†

(3) By a common measure of resistance. We know (29 and 45) how to measure resistances in electromagnetic and electrostatic measure. The ratio between these measures is equal to v^2. The measure of resistance in electrostatic measure is not as yet susceptible of great accuracy.

(4) By a comparison of currents. The electromagnetic value of a current produced by a continuous succession of discharges from a condenser of capacity s can be measured (18, 19). The electrostatic value of the current will be known if the potential to which the condenser is charged be known. The ratio of the two numbers is equal to v.

* Pogg. *Ann.* August 1856, vol. xcix. p. 10. *Abhandlungen der Kön. Sächsischen Gesellschaft*, vol. iii. (1857), p. 266.

† Paper read before the Royal Society, February 1860. *Vide* Proceedings of the Royal Society, vol. x. p. 319, and Phil. Mag. S. 4. vol. xx. p. 233.

(5) By a common measure of capacity. The two measurements can be effected by the methods given (26 and 40). The ratio between the two measurements will give v^2. This method would probably yield very accurate results.

Part V.—*Electrical Measurements derived from the five elementary Measurements; and Conclusion.*

47. *Electric Potential.*—The word "potential," as applied by G. Green to the condition of an electrified body and the space surrounding it, is now coming into extensive use, but is perhaps less generally understood than any other electrical term. Electric potential is defined by Professor W. Thomson as follows*.

"The potential, at any point in the neighbourhood of or within an electrified body, is the quantity of work that would be required to bring a unit of positive electricity from an infinite distance to that point, if the given distribution of electricity remained unaltered."

It will be observed that this definition is exactly analogous to that given of magnetic potential (10), with the substitution of the unit quantity of electricity for the unit magnetic pole. (Analogous definitions might be given of gravitation-potential, heat-potential; and every one of these potentials coexist at every point of space quite independently one of the other.) In another paper† Professor Thomson describes electric potential as follows:—"The amount of work required to move a unit of electricity, against electric repulsion, from any one position to any other position, is equal to the excess of the electric potential of the first position above the electric potential of the second position."

The two definitions given are virtually identical, since the potential at every point of infinity is zero, and it will be seen that the difference of potential defined in the second passage quoted is identical with what we have called the electromotive force between the two points (16 and 27).

When, instead of a difference of potentials, *the potential* simply of a point is spoken of, the difference of potential between the point and the earth is referred to, or, as we might say, the electromotive force between the point and the earth.

The potential at all points close to the surface and in the interior of any simple metallic body is constant; that is to say, no electromotive force can be produced in a single metallic body by mere electrical distribution; the potential *at* the body may there-

* Paper read before the British Association, 1852. *Vide* Phil. Mag. 1853, p. 288.

† Paper read before the Royal Society, February 1860. *Vide* Proceedings of the Royal Society, vol. x. p. 334, and Phil. Mag. S. 4 vol. xx. p. 323.

fore be called the potential *of* the body. The potential of a metallic body varies according to the distribution, dimensions, position, and electrification of all surrounding bodies. It also depends on the substance forming the dielectric.

In any given circumstances, the potential of the body will be simply proportional to the quantity of electricity with which it is charged; but if the circumstances are altered, the potential will vary although the total amount of the charge may remain constant.

In a closed circuit in which a current circulates, the potential of all parts of the circuit is different; the difference depends on the resistance of each part and on the electromotive force of the source of electricity, *i. e.* on the difference of potentials which it is capable of causing when its two electrodes are separated by an insulator or dielectric. The different parts of a conductor moving in a magnetic field are maintained at different potentials, inasmuch as we have shown that an electromotive force is produced in this case. The potential of a body moving in an electric field (*i. e.* in the neighbourhood of electrified bodies) is constantly changing, but at any given moment the potential of all the parts is equal. The use of the word "potential" has the following advantages. It enables us to be more concise than if we were continually obliged to use the circumlocution, "electromotive force between the point and the earth;" and it avoids the conception of a force capable of generating a current, which almost necessarily, although falsely, is attached to "electromotive force."

Equipotential surfaces and lines of force in an electric field may be conceived for statically electrified bodies; these surfaces and lines would be drawn on similar principles and possess analogous properties to those described in a magnetic field (10). It is hardly necessary to observe that the magnetic and the electric fields are totally distinct, and coexist without producing any mutual influence or interference.

The rate of variation of electric potential per unit of length along a line of force is at any point equal to the electrostatic force at that point, *i. e.* to the force which a unit of electricity placed there would experience. The unit difference of potential is identical with the unit electromotive force; and the electrometer spoken of as measuring electromotive force measures potentials or differences of potential.

48. *Density, Resultant Electric Force, Electric Pressure.*— The three following definitions are taken almost literally from a paper by Professor W. Thomson*. Our treatise would be in-

* Paper read before the Royal Society, February 1860. *Vide* Proceedings of the Royal Society, vol. x. p. 333 (1860), and Phil. Mag. S. 4. vol. xx. p. 322.

complete without reference to these terms, and Professor Thomson's definitions can hardly be improved.

"*Electric Density.*—This term was introduced by Coulomb to designate the quantity of electricity per unit of area in any part of the surface of a conductor. He showed how to measure it, though not in absolute measure, by his proof-plane.

"*Resultant Electric Force.*—The resultant force in air or other insulating fluid in the neighbourhood of an electrified body is the force which a unit of electricity concentrated at that point would experience if it exercised no influence on the electric distributions in the neighbourhood. The resultant force at any point in the air close to the surface of a conductor is perpendicular to the surface, and equal to $4\pi\rho$, if ρ designates the electric density of the surface in the neighbourhood.

"*Electric Pressure from the Surface of a Conductor balanced by Air.*—A thin metallic shell or liquid film, as for instance a soap-bubble, if electrified, experiences a real mechanical force in a direction perpendicular to the surface outwards, equal in amount per unit of area to $2\pi\rho^2$, ρ denoting, as before, the electric density at the part of the surface considered. In the case of a soap-bubble its effect will be to cause a slight enlargement of the bubble on electrification with either vitreous or resinous electricity, and a corresponding collapse on being perfectly discharged. In every case we may consider it as constituting a deduction from the amount of air-pressure which the body experiences when unelectrified. The amount of deduction being different at different parts according to the square of the electric density, its resultant action on the whole body disturbs its equilibrium, and constitutes in fact the resultant electric force experienced by the body."

49. *Tension.*—The use of this word has been intentionally avoided by us in this treatise, because the term has been somewhat loosely used by various writers, sometimes apparently expressing what we have called the density, and at others diminution of air-pressure. By the most accurate writers it has been used in the sense of a magnitude proportional to potential or difference of potentials, but without the conception of absolute measurement, or without reference to the idea of work essential in the conception of potential. We believe also that it has not been generally, if ever, applied to that condition of an insulating fluid in virtue of which each point has an electric potential, although no sensible quantity of electricity be present at the point. The expression "tension" might be used to designate what we have termed the potential of a body. The tension between two points would then be equivalent to the electromotive force between those points, or to their difference of potentials, and would be measured in the same unit.

50. *Conducting-Power, Specific Resistance, and Specific Conducting-Power.*

Conducting-Power, or Conductivity.—These expressions are employed to signify the reciprocal of the resistance of any conductor. Thus, if the resistance of a wire be expressed by the number 2, its conducting-power will be 0·5.

Specific Resistance referred to Unit of Mass.—The specific resistance of a material at a given temperature may be defined as the resistance of the unit mass formed into a conductor of unit length and of uniform section. Thus the specific resistance of a metal in the metrical system is the resistance of a wire of that metal, one metre long, and weighing one gramme.

The Specific Conducting-Power of a material is the reciprocal of its specific resistance.

Specific resistance, referred to unit of volume, is the resistance opposed by the unit cube of the material to the passage of electricity between two opposed faces. It may easily be deduced from the specific resistance referred to unit of mass, when the specific gravity of the material is known.

Specific conducting-power may also be referred to unit of volume. It is of course the reciprocal of the specific resistance referred to the same unit.

It is somewhat more convenient to refer to the unit of mass with long uniform conductors, such as metal wires, of which the size is frequently and easily measured by the weight per foot or metre; and it is, on the other hand, more convenient to refer to the unit of volume bodies, such as gutta percha, glass, &c., which do not generally occur as conducting-rods of uniform section, while theird imensions can always be measured with at least as much accuracy as their weights.

51. *Specific Inductive Capacity.*—Faraday* discovered that the capacity of a conductor does not depend simply on its dimensions or on its position relatively to other conductors, but is influenced in amount by the nature of the insulator or dielectric separating it from them. The laws of induction are assumed to be the same in all insulating materials, although the amount be different. The name "inductive capacity" is given to that quality of an insulator in virtue of which it affects the capacity of the conductor it surrounds, and this quality is measured by reference to air, which is assumed to possess the unit inductive capacity. The specific inductive capacity of a material is therefore equal to the quotient of the capacity of any conductor insulated by that material from the surrounding conductors, divided by the capacity of the same conductor in the same position separated

* Experimental Researches, series xi.

from them by air only. It is not improbable that this view of induction may be hereafter modified.

52. *Heat produced in a Conductor by a Current.*—The work done in driving a current, C, for a unit of time through a conductor whose resistance is R, by an electromotive force E, is $EC = RC^2$ (§ 17). This work is lost as electrical energy, and is transformed into heat. As Dr. Joule has ascertained the quantity of mechanical work equivalent to one unit of heat, we can calculate the quantity of heat produced in a conductor in a given time, if we know C and R in absolute measure. In the metrical series of units founded on the metre, gramme, and second, if we call the total heat Θ, taking as unit the quantity required to raise one gramme of water one degree Centigrade, we have

$$\Theta = \frac{RC^2t}{4157}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (28)$$

In the British system, founded on feet, grains, and seconds, with a unit of heat equal to the quantity required to raise one grain of water one degree Fahrenheit, we must substitute the number 24861 for 4157 in the above equation.

53. *Electrochemical Equivalents.*—Dr. Faraday has shown* that when an electric current passes through certain substances and decomposes them, the quantity of each substance decomposed is proportional to the quantity of electricity which passes. Hence we may call that quantity of a substance which is decomposed by unit current in unit time the electrochemical equivalent of that substance.

This equivalent is a certain number of grammes of the substance. The equivalents of different substances are in the proportion of their combining-numbers; and if all chemical compounds were electrolytes, we should be able to construct experimentally a table of equivalents, in which the weight of each substance decomposed by a unit of electricity would be given. The electrochemical equivalent of water, in electromagnetic measure, is about 0·02 in British, 0·0092† in the metrical system. The electrochemical equivalents of all other electrolytes can be deduced from this measurement with the aid of their combining-numbers.

54. *Electromotive Force of Chemical Affinity.*—When two substances having a tendency to combine are brought together and enter into combination, they enter into a new state, in which the intrinsic energy of the system is generally less than it was before; that is, the substances are less able to effect chemical changes, or to produce heat or mechanical action, than before.

* Experimental Researches, series vii.

† ·009375 by Weber and Kohlrausch.

The energy thus lost appears during the combination as heat or electrical or mechanical action, and can in many cases be measured*.

The energy given out during the combination of two substances may, like all other forms of energy, be considered as the product of two factors†—the tendency to combine, and the amount of combination effected. Now the amount of combination may be measured by the number of electrochemical equivalents which enter into combination; so that the tendency to combine may also be ascertained by dividing the energy given out by the number of electrochemical equivalents which enter into combination.

If the whole energy appears in the form of electric currents, the energy of the current is measured by the product of the electromotive force and the quantity of electricity which passes. Now the quantity of electricity which passes is equal to the number of electrochemical equivalents which enter on either side into combination. Hence the total energy given out, divided by this number, will give the electromotive force of combination. Thus, if N electrochemical equivalents enter into combination under a chemical affinity I, and in doing so give out energy equal to W, either as heat or as electrical action, then

$$NI = W.$$

But if W be given out as electrical action, and causes a quantity of electricity Q to traverse a conductor under an electromotive force E, we shall have

$$W = EQ.$$

By the definition of electrochemical equivalents,

$$E = N,$$

therefore

$$I = E;$$

or the force of chemical affinity may in these cases be measured as electromotive force.

This method of ascertaining the electromotive force due to chemical combination, which gives us a clear insight into the meaning and the measurement of "chemical affinity," is due to Professor W. Thomson‡.

The field of investigation presented to us by these considera-

* Report of the British Association, 1850, p. 63; and Phil. Mag. S. 3. vol. xxxii. See papers by Professor Andrews, and Favre and Silbermann, "On the Heat given out in Chemical Action," *Comptes Rendus*, vols. xxxvi. and xxxvii.

† See Rankine "On the General Law of Transformation of Energy," Phil. Mag. 1853.

‡ "On the Mechanical Theory of Electrolysis," Phil. Mag. Dec. 1851.

tions is very wide. We have to measure the intrinsic energy of substances as dependent on volume, temperature, and state of combination. When this is done, the energy due to any combination will be found by subtracting the energy of the compound from that of the components before combination.

As the tendency to increase in volume is measured as pressure, and as the tendency to part with heat is measured by the temperature, so in chemical dynamics the tendency to combine will be properly measured by the electromotive force of combination.

55. *Tables of Dimensions and other Constants:—*

Fundamental Units.

Length = L. Time = T. Mass = M.

Derived Mechanical Units.

$$\text{Work} = W = \frac{L^2M}{T^2}. \quad \text{Force} = F = \frac{LM}{T^2}. \quad \text{Velocity} = V = \frac{L}{T}.$$

Derived Magnetical Units.

Strength of the pole of a magnet .	$m = L^{\frac{3}{2}}\ T^{-1}\ M^{\frac{1}{2}}$
Moment of a magnet	$ml = L^{\frac{5}{2}}\ T^{-1}\ M^{\frac{1}{2}}$
Intensity of magnetic field . . .	$H = L^{-\frac{1}{2}}T^{-1}\ M^{\frac{1}{2}}$

Electromagnetic System of Units.

Quantity of electricity	$Q = L^{\frac{1}{2}} \times M^{\frac{1}{2}}$
Strength of electric current . . .	$C = L^{\frac{1}{2}}T^{-1}\ M^{\frac{1}{2}}$
Electromotive force	$E = L^{\frac{3}{2}}T^{-2}\ M^{\frac{1}{2}}$
Resistance of conductor	$R = L\ T^{-1}$

Electrostatic System of Units.

Quantity of electricity	$q = L^{\frac{3}{2}}\ T^{-1}\ M^{\frac{1}{2}}$
Strength of electric currents . . .	$c = L^{\frac{3}{2}}\ T^{-2}\ M^{\frac{1}{2}}$
Electromotive force	$e = L^{\frac{1}{2}}\ T^{-1}\ M^{\frac{1}{2}}$
Resistance of conductor	$r = L^{-1}T$

Let v be the ratio of the electrostatic to the electromagnetic unit of quantity (35 and 46); then $v = 310{,}740{,}000$ metres per second approximately, and we have

$$q = vQ \quad \Big| \quad c = vC \quad \Big| \quad e = \frac{1}{v}E \quad \Big| \quad r = \frac{1}{v^2}R \quad \Big| \quad s = v^2S$$

TABLE for the Conversion of British (foot-grain-second) System to Metrical (metre-gramme-second) System.

	Number of metrical units contained in a British unit.	Log.	Log.	Number of British units contained in a metrical unit.
1. For M	0·0647989	2·8115678	1·1884321	15·43235
2. For L, $\frac{v}{l}$, R, $\frac{1}{r}$, and V.	0·3047945	$\bar{1}$·4840071	0·5159929	3·280899
3. For F (also for foot-grains and metre-grammes)	0·0197504	$\bar{2}$·2955749	1·7044250	50·6320
4. For W	0·0060198	$\bar{3}$·7795820	2·2204179	166·1185
5. For H and electro-chemical equivalents.	0·461085	$\bar{1}$·6637804	0·3362196	2·16880
6. For Q, C, and e.....	0·140536	$\bar{1}$·1477874	0·8522125	7·11561
7. For E, m, q, and c ...	0·0428346	$\bar{2}$·6317949	1·3682051	23·3456
8. For heat.....	0·0359994	$\bar{2}$·5562953	1·4437046	27·7782

British System.—Relations between Absolute and other Units.

One absolute unit of { force / work } = 0·0310666 weight of a grain / foot-grains in London.

In London { weight of a grain / one foot-grain } = 32·1889 absolute units of force / work.

One absolute unit of { force / work } = $\frac{1}{g}$ unit weight / unit weight × unit length everywhere.

g in British system = 32·088 $(1 + 0{\cdot}005133 \sin^2 \lambda)$, where λ = the latitude of the place at which the observation is made.

Heat.—The unit of heat is the quantity required to raise the temperature of one grain of water at its maximum density 1° Fahrenheit.

Absolute mechanical equivalent of unit of heat = 24861 = 772 foot-grains at *Manchester*.

Thermal equivalent of an absolute unit of work = 0·000040224.

Thermal equivalent of a foot-grain at Manchester = 0·0012953.

Electrochemical equivalent of water = 0·02, nearly.

Metrical System.—Relation between Absolute and other Units.

One absolute unit of { force / work } = 0·0809821 weight of a gramme / metre-gramme at Paris.

At Paris { the weight of a gramme / or metre-gramme } = 9·80868 absolute units of force / work.

One absolute unit of { force / work } = $\frac{1}{g}$ unit weight / unit weight × unit length everywhere.

g in metrical system = 9·78021$(1 + 0{\cdot}005133 \sin^2 \lambda)$, where λ = the latitude of the place where the experiment is made.

Heat.—The unit of heat is the quantity required to raise one gramme of water at its maximum density 1° Centigrade.

Absolute mechanical equivalent of the unit of heat $=4157{\cdot}25$ $=423{\cdot}542$ metre-grammes at *Manchester*.

Thermal equivalent of an absolute unit of work $=0{\cdot}00024054$.

Thermal equivalent of a metr-grm. at Manchester $=0{\cdot}00236154$.

Electrochemical equivalent of water $=0{\cdot}0092$, nearly.

56. *Note to the Table of Dimensions, by Professor Clerk Maxwell.*—All the measurements of which we have hitherto treated are supposed to be made in the same medium—ordinary air; but Faraday has shown that other media have different properties. Paramagnetic bodies, such as oxygen and salts of iron, when placed in media less paramagnetic than themselves, behave as paramagnetic bodies; but when placed in media more paramagnetic than themselves, they behave as diamagnetic bodies.

Hence magnetic phenomena are influenced by the nature of the medium in which the bodies are placed, and the system of units and of measurements which we adopt depends on the nature of the medium in which our experiments are made. If we made our experiments in highly condensed oxygen, magnets would attract each other less, and currents would attract each other more, than they do in common air; and the reverse would be the case if we worked in a sea of melted bismuth.

Now if we take into account the "coefficient of magnetic induction" of the medium in which we work, and instead of assuming that of common air to be unity, assume it proportional to the density of that part of the medium to which the magnetic action is due, we shall have the repulsion of two poles $=\frac{mm'}{\mu r^2}$, where mm' are the two poles, μ the density of the magnetic medium, and r the distance. Now a density is a mass, M_1, divided by L^3, the unit of volume. Hence the dimensions of m are $\sqrt{\frac{MM_1}{T^2}}$; or if we can measure the density of the magnetic medium in the same unit of mass as that employed for other purposes, the dimensions of m will be simply $\frac{M}{T}$; those of H will then be $\frac{L}{T}$, or a velocity.

If we suppose the density of the magnetic medium to be taken account of in the electromagnetic units, their dimensions become

Quantity of electricity .	$Q=L^2$, or equivalent to an area.
Strength of current . .	$C=\frac{L^2}{T}$
Electromotive force . .	$E=\frac{M}{T^2}$
Resistance of conductor	$R=\frac{M}{L^2T}$

The electromagnetic unit of quantity of electricity is equal to the electrostatic unit multiplied by a certain velocity, depending on the elasticity of the magnetic medium, and proportional or probably equal to the velocity of propagation of vibrations in it. Hence the dimensions of

Electrostatic quantity	$q=\mathrm{LT}$
Electrostatic current	$c=\mathrm{L}$
Electrostatic electromotive force .	$e=\frac{\mathrm{LM}}{\mathrm{T}^3}$
Resistance	$r=\frac{\mathrm{M}}{\mathrm{T}^3}$

As we have no knowledge of the density, elasticity, &c., of the magnetic medium, we assume it as having a standard state in common air; and supposing all measurements to be made in air, the original table of dimensions is sufficient for expressing measurements made according to one system in terms of any other system.

57. *Magnitude of Units and Nomenclature.*—In connexion with the system of measurement explained in this treatise, two points hitherto unmentioned deserve attention—first, the absolute magnitude of the units, and secondly the nomenclature.

The absolute magnitude is in most cases an inconvenient one, leading to the use either of exceedingly small or exceedingly large numbers. Thus the units of electromagnetic resistance and electromotive force and quantity, and of electrostatic currents, are inconveniently small; the unit of electrostatic resistance is inconveniently large. Decimal multiples and submultiples of these units will therefore probably have to be adopted in practice. The choice of these multiples and submultiples forms part of the business of the Committee.

The nomenclature hitherto adopted is extremely defective. In referring to each measurement, we have to say that the number expresses the value in electrostatic or electromagnetic absolute units: if a multiple is to be used, this multiple will also have to be named; and further, the standard units of length, mass, and time have to be referred to, inasmuch as some writers use the pound and some the grain, some the metre and some the millimetre, as fundamental units. This cumbrous diction, and the risk of error imported by it, would be avoided if each unit received a short distinctive name in the manner proposed by Sir Charles Bright and Mr. Latimer Clark, in a paper read before the British Association at Manchester, 1861.

LXXI. *On Lake-Basins.*

By John Carrick Moore, *Esq., F.G.S., &c.**

PROFESSOR RAMSAY, in his able memoir in defence of his Glacial Theory of Lake-Basins, in the April Number of this Magazine, lays down principles of the erosion produced by a sliding body which, with the greatest deference, I cannot believe to be sound. His words are, "Every physicist knows that when such a body as glacier-ice descends a slope, the direct vertical pressure of the ice will be proportional to its thickness and weight and the angle of the slope over which it flows. If the angle be 5°, the weight and erosive force of a given thickness of ice will be so much, if 10° so much less, 20°, less still, till at length, if we imagine the fall to be over a vertical fall of rock, the pressure against the wall (except accidentally) will be *nil*. But when the same vast body of ice has reached the plain, then motion and erosion would cease, were it not for pressure from behind." By "the direct vertical pressure of the ice," the Professor means that *resolved* portion of the weight which is at right angles to the slope; and this resolved portion, which is stated rather loosely to be proportional to the angle, is proportional to the cosine of the angle, a function which up to 90° diminishes as the angle increases. It does not appear to have struck Professor Ramsay as strange, that by his theorem the erosive force is nothing at 90°, comes into operation as the angle declines from 90°, goes on increasing *sine limite* as the angle diminishes, and just when we expect it to be a maximum, we are told it is *nil* as the angle vanishes. It seems to me that Professor Ramsay has taken a wrong measure of the erosive force. He says "the weight or erosive force," as if the words were equivalent. But mere weight does not erode; weight in motion will. A body sliding down a slope will tend to erode with a force compounded of the pressure perpendicular to the slope and the velocity. Now the velocity of sliding ice (as has been shown by Hopkins) is nearly uniform, and therefore may be taken as proportional to the force in the direction of the motion—that is, as the sine of the inclination θ; therefore the erosive force is as the pressure vertical to the plane $\times \sin\theta$; that is, as weight $\times \cos\theta \times \sin\theta$; that is, as weight $\times \sin 2\theta$. This expression is in accordance with Professor Ramsay's theory, that when the angle is 90°, the erosion is 0; and again, when the angle is 0, the erosion is nothing: but it is quite discordant from his view, that the erosion is greater at an

* Communicated by the Author.

angle of 5° than at one of 10°, &c. In fact the maximum will be at 45°; and this I believe to be in accordance with what takes place in rivers with highly-inclined beds. At steep rapids the erosion is considerable; but when the angle becomes almost imperceptible, the river, so far from cutting down its bed, often raises it by deposition of sediment.

As soon as the glacier reaches the plain, erosion by sliding ceases; and if it moves, it must be by propulsion; and if it excavates, the materials ground down must be removed. It is difficult to conceive how this can have been effected but by running water; and that is contrary to the idea of a rock-basin. In those cases where a glacier has been seen to be forced up a slope, is it certain that the rock with the same slope extended quite across the valley? May not one side of the bottom of the valley have been higher than the other? so that while the ice was forced up the slope on one side, the rest of the glacier with water issuing from under it may have been sliding down the other side. But if there is to be no river, then how were, say, the last 100 feet of depth of the Lake of Geneva excavated? It is not "le premier pas qui coûte," but "le dernier." Even granting that the enormous mass which the problem supposes could be forced up a slope, what becomes of the fine fluid mud into which the rocky contents of the lake had been ground? The advancing face of the glacier cannot be presumed to have forced the water before it, for it is fissured in all directions; and though a glacier is said sometimes to thrust pebbles before it, the watery mud would always subside into the depths.

Professor Ramsay, in his memoir published last October, admits that a quasi-plastic body constantly pressed from behind, when opposed by a high impassable barrier like the Jura, would spread itself out in the direction of least resistance. On this I would observe that it was not the *height* of the Jura which formed the impassable barrier: the glacier merely felt the resistance of a rock *at its own level*, and, in obedience to the law, which I cordially accept, turned aside in the direction of least resistance. And on the same principle I should expect the Rhone glacier, on issuing from the gorge, to crawl along the plain, as glaciers are known to do, instead of seeking out resistance by burying itself 1000 feet among hard rocks.

LXXII. *Chemical Notices from Foreign Journals.* *By* E. Atkinson, *Ph.D., F.C.S.*

[Continued from p. 380.]

HERRMANN* has investigated the action of nascent hydrogen on benzoic acid. When an excess of this acid in water is heated nearly to boiling, and sodium-amalgam added, the nascent hydrogen is almost entirely consumed; and if neutralization and solution of the acid are prevented by the gradual addition of hydrochloric acid, the following changes occur: the liquid smells strongly of oil of bitter almonds; during the addition of the hydrochloric acid benzoic acid separates along with an oily body; as the reaction continues, the former finally disappears, while the latter alone remains in solution.

When, in the course of the above reaction, the liquid had become alkaline, it was distilled until the odour of oil of bitter almonds was no longer perceived. In this way an aqueous distillate was obtained containing oil-drops; on shaking this with ether, and subsequently distilling off the ether, an oily liquid was obtained which proved to be benzylic alcohol, $C^{14}H^8O^2$, the formation of which from benzoic acid is thus expressed:—

$$C^{14}H^6O^4 + 4H = 2HO + C^{14}H^8O^2.$$

By exhausting the alkaline residue in the above case with ether, a heavy aromatic oil was obtained, which after some time solidified to a crystalline magma.

The liquid from which this had been extracted by ether was returned to the retort, and again treated with sodium-amalgam until an oil was separated which did not solidify in the cold.

The above crystalline substance, when boiled with water, was partly dissolved; on cooling, a body was obtained which crystallized in laminæ. The melting-point of this substance is 116°; it is unaltered by boiling with potash, but by destructive distillation is resolved completely into oil of bitter almonds. The body has the formula $C^{28}H^{14}O^4$, and is isomeric with hydrobenzoin, obtained by Zinin by the action of zinc and sulphuric acid on oil of bitter almonds. From its formula it might be a compound of benzoic aldehyde with an aldehyde richer by two atoms of hydrogen.

The third substance separated from the alkaline solution by hydrochloric acid is a volatile oily acid with an extremely unpleasant odour, strongly suggestive of valerianic acid. It forms salts, which cannot be crystallized, but readily attract moisture, and are decomposed on exposure to the air in consequence of oxidation of the acid. As it was impossible to get good results

* Liebig's *Annalen*, October 1864.

by the analysis of the acid or of the salts, the ether was made, and thus the formula of the acid was found to be $C^{14}H^{10}O^4$. It contains hence four atoms of hydrogen more than benzoic acid; and it forms an intermediate member between the acids which correspond, as regards carbon, in the aromatic and fatty acid series :—

Benzoic acid	$C^{14}H^6O^4$
Intermediate acid . . .	$C^{14}H^{10}O^4$
Œnanthylic acid . . .	$C^{14}H^{14}O^4$

Herrmann calls this acid *Benzoleic acid*; it is distinguished from the fatty acids only by greater specific gravity.

When monosulphide of ethyle and iodide of ethyle are heated together under suitable conditions, Von Oefele* has found that they combine and form a compound crystallizing in small laminæ which contains the elements of one atom of sulphide of ethyle and one atom of iodide of ethyle. Its chemical deportment leads to the view that it is the iodide of a radical consisting of three atoms of ethyle and two of sulphur, $\left.\begin{matrix}C^4H^5\\C^4H^5\\C^4H^5\end{matrix}\right\}S^2$, which Oefele calls *Triethylsulfine*. The body can also be obtained by distilling a mixture of alcoholic solution of monosulphide of potassium with iodide of ethyle.

When the aqueous solution of the iodide is treated with nitrate of silver, iodide of silver is at once precipitated, and the solution contains *nitrate of triethylsulfine*; or if it is treated with freshly precipitated oxide of silver, iodide of silver is obtained with hydrated oxide of triethylsulfine, $\left.\begin{matrix}C^4H^5\\C^4H^5\\C^4H^5\end{matrix}\right\}S^2O, HO$. The solution filtered off from the iodide of silver has a strongly alkaline reaction, readily attracts carbonic acid, precipitates solutions of the metals just as does potash, and, when evaporated in the exsiccator or in a vacuum, yields a crystalline body. It so readily attracts moisture that an analysis could not be made. Its salts are obtained by neutralization with the corresponding acid; they are difficult to crystallize, and are very deliquescent. A platinum-salt, however, was obtained, in splendid dark reddish-yellow prisms, which had the composition

$$\left.\begin{matrix}C^4H^5\\C^4H^5\\C^4H^5\end{matrix}\right\}S^2Cl + PtCl^2.$$

* Liebig's *Annalen*, October 1864.

Oefele has also found* that when monosulphide of ethyle is gradually added to fuming nitric acid, an energetic action ensues, the result of which is the formation of a body which crystallizes readily in thin large-sized colourless plates. This body has the composition $C^8 H^{10} S^2 O^4$, and Oefele names it *Diethylsulphan*, considering its composition to be $\left.\begin{matrix} C^4 H^5 \\ C^4 H^5 \end{matrix}\right\} S^2 O^4$, analogous to sulphobenzole, $\left.\begin{matrix} C^{12} H^5 \\ C^{12} H^5 \end{matrix}\right\} S^2 O^4$, which he names diphenylsulphan. Diethylsulphan melts at 70°, and boils at 248° C., distilling without decomposition. Treated with nascent hydrogen, it is reduced to monosulphide of ethyle.

Vogt† showed that the chloride of phenylsulphuric acid, by the action of nascent hydrogen, yielded phenylmercaptan. Schertel‡ shows that by applying this reaction to the chloride of naphthylsulphuric acid (which Kolbe and his pupils name chloride of naphthyl-sulphan (*Naphthylsulfanchlorid*)), the corresponding mercaptan of naphthylic alcohol is obtained, a change expressed by the following equation,

$$\underset{\text{Chloride of naphthylsulphan.}}{C^{20} H^7 S^2 O^4 Cl} + 6\,H = \underset{\text{Naphthylic mercaptan.}}{C^{20} H^8 S^2} + HCl + 4\,HO.$$

The change is effected by adding the substance to a mixture of zinc and sulphuric acid from which hydrogen is being disengaged. On distilling the liquid, an oil passes over, which, after appropriate purification, has the following properties:—colourless, highly refringent, of unpleasant but not intense odour, not miscible with water, but soluble in alcohol and ether. It has the specific gravity 1·146, and boils at 285° C. Like other mercaptans, it readily exchanges an atom of hydrogen for metals. The naphthylmercaptide of mercury, $\left.\begin{matrix} C^{20} H^7 \\ Hg \end{matrix}\right\} S^2$, is a light pale yellow powder; the corresponding compounds of copper, $\left.\begin{matrix} C^{20} H^7 \\ Cu \end{matrix}\right\} S^2$, and of lead, $\left.\begin{matrix} C^{20} H^7 \\ Pb \end{matrix}\right\} S^2$, are also insoluble precipitates.

When naphthylmercaptan is dissolved in strong ammonia and the solution left to spontaneous evaporation, after some days transparent well-defined crystals are formed, belonging to the klinorhombic system. These are the bisulphide of naphthyle, $C^{20} H^7 S^2$, the production of which, supposing the previous

* Liebig's *Annalen*, October 1864, p. 86.
† Phil. Mag. S. 4. vol. xxii. p. 302.
‡ Liebig's *Annalen*, October 1864.

formation of naphthylmercaptan ammonium, takes place in consequence of oxidation; thus

$$\left.\begin{matrix} C^{20}H^{7} \\ NH^{4} \end{matrix}\right\} S^{2} + O = C^{20}H^{7}S^{2} + HO + NH^{3}.$$

Napthylmercaptan ammonium. Bisulphide of naphthyle.

Linnemann* has continued his investigations† of benzophenone, $C^{13}H^{10}O$, the acetone corresponding to benzoic acid. He finds that benzoate of lime, when distilled with lime, yields this body along with benzole in about the proportion of two of the latter for one of the former. Benzophenone has a great tendency to crystallization, and is readily obtained in large crystals. It unites with bromine, but not without the formation of hydrobromic acid, and the bodies formed are not of a very definite nature.

Linnemann had previously found that by the action of nascent hydrogen on benzophenone a body was formed which he called *Benzhydrole*, and assigned to it the formula $C^{13}H^{14}O$, which subsequent analyses show ought to be $C^{13}H^{12}O$. This body has many of the properties of a monatomic alcohol, but differs in that, when oxidized (as, for example, by chromic acid) it reproduces benzophenone, and not an aldehyde. If the benzhydrole is treated by strong nitric acid, oxidation first takes place, forming benzophenone, which is then converted into *binitrobenzophenone*, $C^{13}H^{8}(NO^{2})^{2}O$.

Benzophenone thus behaves exactly like acetone. That substance, when treated by nascent hydrogen, yields an alcohol isomeric but not identical with propylic alcohol, and called accordingly isopropylic alcohol. This isopropylic alcohol when oxidized yields the body acetone from which it was formed, instead of propylic aldehyde as is the case with ordinary propylic alcohol. Benzhydrole also differs in another important respect from ordinary alcohol, inasmuch as it readily forms with bromine *bibrominated benzhydrole*, $C^{13}H^{10}Br^{2}O$. This body consists of a light glistening white mass, which is seen, under the microscope, to consist of small acicular crystals.

Benzhydrole resembles the alcohols in that it readily forms compound ethers; it differs in that, as yet, it has not been possible to form a chloride or iodide of the alcohol. The action of the chloride of phosphorus is simply a withdrawal of water with the formation of *benzhydrolic ether*, $\left.\begin{matrix} C^{13}H^{11} \\ C^{13}H^{11} \end{matrix}\right\} O$. This body is also formed by keeping benzhydrole for some time at the boil-

* Liebig's *Annalen*, January 1864.

† Phil. Mag. S. 4. vol. xxv. p. 539.

ing-temperature under the ordinary atmospheric pressure; it is then split up into the ether and water, thus,

$$2\left.\begin{matrix}\mathrm{C^{13}H^{11}}\\ \mathrm{H}\end{matrix}\right\}\mathrm{O}=\left.\begin{matrix}\mathrm{C^{13}H^{11}}\\ \mathrm{C^{13}H^{11}}\end{matrix}\right\}\mathrm{O}+\mathrm{H^2O}.$$

It is the first instance of a monatomic alcohol being decomposed in this way by the simple action of heat. It is obtained in clear adamantine crystals belonging to the rhombic system.

When to benzhydrole dissolved in absolute alcohol a small quantity of sulphuric acid is added and the mixture left for some days, an oil is separated on the subsequent addition of water, which has the formula $\mathrm{C^{15}H^{16}O}$. It is a mixed ether, *ethyl-benzhydrolic ether*, $\left.\begin{matrix}\mathrm{C^{13}H^{11}}\\ \mathrm{C^2H^5}\end{matrix}\right\}\mathrm{O}$, and is a colourless liquid of the consistency of glycerine. This body exhibits a remarkable deportment towards light. Shortly after distillation it is a colourless liquid; but kept, it becomes coloured. This colour arises from the action of light; for when the liquid is kept for some time in the dark it disappears. The coloration reappears after some time when brought into diffused daylight, but immediately when brought into the sunlight. By incident light the liquid appears of a beautiful green, and by transmitted of a pale yellow. The colour, besides depending on the light, appears to depend on a certain position of rest, for it disappears when the liquid is moved backwards and forwards. It is also lost by warming. If the colour has been destroyed by exposure to dark, by agitation, or by heat, it may be brought back by exposure to direct sunlight. Yet, strangely enough, this condition is transient; for when the liquid has been kept for some months it is colourless, and does not regain its previous properties, even by distillation. But in any condition of the liquid, when a cone of light is transmitted through it, it is highly fluorescent, the convergent light being of a beautiful blue.

Besides acetic benzhydrolic ether (which has also these remarkable luminous properties) and benzoic benzhydrolic ether which Linnemann formerly described, and which description he now completes, he has prepared other ethers. The succinic benzhydrolic ether, $\left.\begin{matrix}(\mathrm{C^{13}H^{11}})^2\\ \mathrm{C^4H^4O^2}\end{matrix}\right\}\mathrm{O^2}$, consists of small lustrous laminæ which melt at 142°.

This compound experiences, by the action of heat, a singular change, which is expressed by the following equation:—

$$\left.\begin{matrix}(\mathrm{C^{13}H^{11}})^2\\ \mathrm{C^4H^4O^2}\end{matrix}\right\}\mathrm{O^2}=\left.\begin{matrix}\mathrm{C^4H^4O^2}\\ \mathrm{H^2}\end{matrix}\right\}\mathrm{O^2}+2\,\mathrm{C^{13}H^{10}}.$$

It is decomposed into succinic acid and a hydrocarbon which,

when appropriately purified, forms small well-shaped rhombic plates of considerable thickness. It melts at 209° to 210° C.

When, to an alcoholic solution of benzophenone, sulphuric acid is added and then zinc, a slow disengagement of hydrogen takes place; and after some time a substance difficultly soluble in alcohol forms, and coats the zinc like a white crust. When this is purified it is obtained as a brilliant white mass, consisting of small needles. It has the formula $C^{26} H^{22} O^2$, and is derived from two molecules of benzophenone by the assimilation of two of H. It stands to benzophenone just in the same relation as pinakone to acetone. Thus,

$$2\,C^3 H^6 O + H^2 = C^6 H^{14} O^2.$$
Acetone. Pinakone.

$$2\,C^{13} H^{10} O + H^2 = C^{26} H^{22} O^2.$$
Benzophenone.

Linnemann has assigned to it the provisional name *benzpinakone*.

Standing, as regards its composition, between benzophenone and benzhydrole, it is readily converted into either; thus, boiled with a dilute solution of chromic acid, it yields the former,

$$C^{26} H^{22} O^2 + O = 2\,C^{13} H^{10} O + H^2 O;$$
Benzpinakone. Benzophenone.

while by the reducing action of sodium-amalgam it is changed into the latter,

$$C^{26} H^{22} O^2 + H^2 = 2\,C^{13} H^{12} O.$$
Benzhydrole.

Benzpinakone can neither be distilled nor melted without an alteration of its physical properties. In both cases it is converted into a body isomeric with benzpinakone, and which is called *isobenzpinakone*. This is a colourless syrupy liquid which boils at 297°·5, and does not solidify even at −15°. By keeping, however, for a few months at the ordinary temperature it becomes solid. Both modifications of benzpinakone have the same chemical reactions.

The above investigations show that the hydrogen disengaged from an acid solution acts quite differently to that from an alkaline solution. A similar investigation of the behaviour of acroleine towards zinc and hydochloric acid, by Linnemann*, has shown that the products here obtained also differ from those produced by hydrogen disengaged in alkaline solutions. The action on the acid is very energetic, and requires to be moderated by mixing an ice-cold ethereal solution of acroleine with equally cold hydrochloric acid, gradually adding the mixture to

* Liebig's *Annalen*, January 1865.

the zinc. Two of the bodies produced are allylic and propylic alcohol, which remain dissolved in the aqueous solution of chloride of zinc; besides these, there is an insoluble substance which partly forms a deposit and partly is dissolved in an ethereal layer which floats on the liquid. By exhausting the entire liquid with ether, the whole of this substance is obtained in solution.

The solution of chloride of zinc containing the two alcohols was distilled, and the distillate dehydrated by carbonate of potash and again distilled, and this distillate dehydrated by anhydrous sulphate of copper. In this way a volatile liquid was obtained, consisting of about one volume of propylic and six of allylic alcohol. It was impossible to separate these completely by fractional distillation, for their boiling-points are so near, though ample evidence of the identity of the two substances was obtained from their reactions; and to effect the complete separation, the mixed alcohols were treated by hydriodic acid, by which they were converted into iodides. Iodide of allyle, $C^3 H^5 I$, readily unites with mercury to form a compound, $C^3 H^5 I Hg^2$, while iodide of propyle is unaffected by mercury: advantage was taken of this deportment to separate the two iodides.

The properties of the iodide of allyle were found identical with those of that prepared from glycerine. From the iodide of propyle, $C^3 H^7 I$, propylic alcohol was obtained identical with that obtained from acetone, and which is therefore *isopropylic* alcohol.

The ethereal solution contained the above-mentioned insoluble substance, and also some chlorine and sulphur organic compounds. When, however, the ethereal solution was evaporated, these latter became carbonized and completely decomposed; and hence the first purification consisted in distilling and redistilling the liquid until it passed over without decomposition. An oily liquid was left, which could not be obtained of quite constant boiling-point; freshly distilled it is colourless, but rapidly becomes brown on standing, and has a peculiar camphorous odour. It has the formula $C^6 H^{10} O^2$, and stands to acroleine in the same relation that pinakone and benzpinakone do respectively to acetone and benzophenone, thus,

$$\underset{\text{Acroleine.}}{2C^3 H^4 O} + H^2 = \underset{\text{New body.}}{C^6 H^{10} O^2}.$$

The chemical relations of this new substance, to which the name *acropinakone* is provisionally assigned, have not yet been completely examined, and the author is still engaged upon the subject. His researches show that the action of the hydrogen from an acid solution on acroleine gives rise to three bodies, whose formation may be thus collated:—

$$\mathrm{C^3\,H^4\,O + H^2 = C^3\,H^6\,O.}$$
Allylic alcohol.

$$\mathrm{C^3\,H^4\,O + 2\,H^2 = C^3\,H^8\,O.}$$
Propylic alcohol.

$$\mathrm{2\,C^3\,H^4\,O + H^2 = C^6\,H^{10}\,O^2.}$$
Acropinakone.

Linnemann* has also found that pinakone exists in two isomeric modifications—a liquid one, which boils at 176° or 177°, and dissolves in water, from which it soon separates as a crystalline hydrate; the solid pinakone is a snow-white crystalline mass which melts at 35° to 38°, gradually softening first, and boils at 171° to 172°. The distillate is a colourless thickish liquid, which soon solidifies. Both these modifications unite with water and form one and the same hydrate, $\mathrm{C^6\,H^4\,O^2 + 6\,Aq}$; pinakone forms besides, as Fittig has shown, several hydrates.

Freund found some time ago† that the synthesis of acetone could be effected by the action of chloride of acetyle on zinc-methyle; thus

$$\mathrm{Zn''\left\{\begin{matrix}C\,H^3\\ C\,H^3\end{matrix}\right\} + 2\,C^2\,H^3\,O\,Cl = Zn''\,Cl^2 + 2\,C^3\,H^6\,O.}$$
Zinc-methyle. Chloride of acetyle. Chloride of zinc. Acetone.

Friedel‡ has now found that the synthesis of acetone can also be accomplished by another reaction, in which he makes use of chloracetene, $\mathrm{C^2\,H^3\,Cl}$, the interesting body obtained by Harnitz-Harnitzsky§, by the action of phosgene gas on aldehyde. When this substance acts upon methylate of sodium, chloride of sodium is formed and acetone; thus,

$$\mathrm{C^2\,H^3\,Cl + C\,H^3\,Na\,O = NaCl + C^3\,H^6\,O.}$$

A quantity of sodium corresponding to the chloracetene was dissolved in methylic alcohol and the excess evaporated. On gradually adding chloracetene to the mixture, kept cool, a brisk reaction was established, chloride of sodium was formed, and, when the mass was distilled, a distillate was obtained consisting of acetone mixed with a little methylic alcohol. As the boiling-points of the two liquids are very close, separation could not be effected by fractional distillation, and the mixture was accordingly treated by permanganate of potash, by which methylic alcohol is oxidized very much more readily than acetone. In this

* Liebig's *Annalen*, (Suppl. 3) vol. iii. part 3.
† Phil. Mag. S. 4. vol. xx. p. 201.
‡ *Comptes Rendus*, May 1, 1865.
§ Phil. Mag. S. 4. vol. xvii. p. 430.

way acetone was obtained pure, as evidenced by its boiling-point, its analysis, and its combining with sulphite of soda.

Borsche and Fittig*, in an investigation of acetone, describe some of its derivatives, and a new mode of converting that body into allylene gas†. When chlorine is passed into acetone, hydrochloric acid is formed, which remains dissolved in the acetone. On distilling the liquid, torrents of hydrochloric acid are given off, and a distillate is obtained consisting of unchanged acetone along with *dichlorinated acetone*, $C^3 H^4 Cl^2 O$, a body which, when suitably purified, has a pleasant ethereal odour, and boils at 120°.

When this body is treated with pentachloride of phosphorus, it is acted on in the same way as is acetone; that is, it exchanges its atom of oxygen for two of chlorine.

The decomposition takes place according to the equation

$$\underset{\text{Dichlorinated acetone.}}{C^3 H^4 Cl^2 O} + PCl^5 = \underset{\text{Chloride of dichlorinated acetone.}}{C^3 H^4 Cl^4} + PCl^3 O.$$

The reaction requires, however, for its completion a lengthened action of the pentachloride. The new compound, $C^3 H^4 Cl^4$, is a colourless oil of not unpleasant odour, which boils at 153°. It is isomeric with Cahours's quadrichlorinated propylene, the boiling-point of which, however, is about 50° higher.

When chloride of dichlorinated acetone is treated with either caustic potash or ammonia, it is resolved into a body, $C^3 H^3 Cl^3$, which is isomeric but not identical with trichlorinated propylene, and which Borsche and Fittig call *isotrichlorinated propylene*.

From its formula, chloride of dichlorinated acetone can be regarded as the chloride of a tetratomic hydrocarbon, $C^3 H^{4'''''} Cl^4$; this is, in fact, the case; the hydrocarbon is the gas allylene. The action of sodium on the chloride is at first feeble, soon begins to be extremely violent, and requires to be moderated. It was found best to dissolve the chloride in four times its volume of rectified benzole before adding the sodium; by warming or by cooling, the action could then either be accelerated or moderated. The allylene was given off as gas, and was absorbed by passing it into an ammoniacal solution of subchloride of copper, with which it forms a well-defined compound—copper-allylene. When this compound was treated with hydrochloric acid, allylene gas was disengaged, the identity of which was determined by converting it into the bromide, $C^3 H^4 Br^2$, and the tetrabromide, $C^3 H^4 Br^4$, already described by Oppenheim‡.

* Liebig's *Annalen*, January 1865. † Vide *antea*, p. 306.
‡ Ibid. p. 307.

Harnitz-Harnitzsky, who succeeded in effecting the synthesis of benzoic acid by adding carbonic acid to benzole*, has now† extended this reaction to the series of hydrocarbons homologous with marsh-gas, and has thus artificially prepared acetic acid, and its homologue, caproic acid.

Synthesis of Acetic Acid.—When marsh-gas was passed, together with oxychloride of carbon, into a vessel heated to 120°, a reaction took place which is represented by the following equation:—

$$\underset{\text{Marsh-gas.}}{C H^4} + \underset{\text{Phosgene gas.}}{C O Cl^2} = \underset{\text{Chloride of acetyle.}}{C^2 H^3 O Cl} + HCl.$$

Some of the chloride of acetyle thus formed was condensed in the apparatus, and by its properties can be readily identified; but the greater part was collected in a vessel connected with the receiver, and containing caustic soda, by which it was completely decomposed, forming chloride of sodium and acetic acid, which then united with soda, forming acetate of soda. The decomposition of chloride of acetyle by the soda is thus expressed:—

$$\underset{\text{Chloride of acetyle.}}{C^2 H^3 O Cl} + NaHO = \underset{\text{Acetic acid.}}{C^2 H^4 O^2} + NaCl.$$

In like manner Harnitz-Harnitzsky has prepared chloride of caproyle by the action of oxychloride of carbon on hydride of amyle; and this, by its action on water, gave caproic acid, thus,

$$\underset{\text{Hydride of amyle.}}{C^5 H^{12}} + \underset{\text{Chlorocarbonic acid.}}{C O Cl^2} = \underset{\text{Chloride of caproyle.}}{C^6 H^{11} O Cl} + HCl;$$

$$C^6 H^{11} O Cl + H^2 O = \underset{\text{Caproic acid.}}{C^6 H^{12} O^2} + HCl.$$

Under the class of *acetals* may be ranged all compounds of aldehydes with monoatomic alcohols. They have their origin in the combination of two equivalents of alcohol with one of aldehyde, with loss of water. Wurtz has described the existence of a similar compound, produced by the union of glycol with aldehyde under elimination of water. Harnitz-Harnitzsky and Menschutkin‡ have completed this group, by announcing the existence of a similar class of bodies derived from the triatomic alcohols the glycerines, and which they call *glycerals*. They

* Vide *antea*, p. 310.
† *Comptes Rendus*, May 1, 1865.
‡ Ibid., March 20, 1865.

are formed in accordance with the following equation:—

$$\underset{\text{Glycerine.}}{\text{C}^3\text{H}^8\text{O}^3} + \text{aldehyde} - \text{H}^2\text{O} = \text{glyceral.}$$

Acetoglyceral, $\left.\begin{matrix}\text{C}^3\text{H}^5\\ \text{H}\\ \text{C}^2\text{H}^4\end{matrix}\right\}\text{O}^3$, is formed by heating glycerine with aldehyde in closed tubes for thirty hours to a temperature of 170°–180°. On distilling the contents of the tubes, a body is obtained which passes over between 184°–188°, and is the substance in question. It is a dense liquid, slightly soluble in water. Freshly distilled it is almost inodorous; but exposed to the moisture of the air, the odour of acetic aldehyde soon appears.

Valeroglyceral, $\left.\begin{matrix}\text{C}^3\text{H}^5\\ \text{H}\\ \text{C}^5\text{H}^{10}\end{matrix}\right\}\text{O}^3$, and *benzoglyceral*, $\left.\begin{matrix}\text{C}^3\text{H}^5\\ \text{H}\\ \text{C}^7\text{H}^6\end{matrix}\right\}$, have been prepared by analogous methods.

In his general investigations on organic acids, Kekulé had observed that, by the action of sodium-amalgam on aconitic acid, an acid richer in hydrogen was produced. This observation has been pursued by Wichelhaus*, who has found that the acid thus formed is identical with an acid obtained by Maxwell Simpson by the action of potash on cyanide of allyle: the acid received no name from its discoverer, but Kekulé has proposed for it the name *carballylic* acid. The formation of this acid from aconitic acid is thus expressed:—

$$\underset{\text{Aconitic acid.}}{\text{C}^6\text{H}^6\text{O}^6} + 2\,\text{H} = \underset{\text{Carballylic acid.}}{\text{C}^6\text{H}^8\text{O}^6}.$$

LXXIII. *On Change of Climate due to the Excentricity of the Earth's Orbit.*

To the Editors of the Philosophical Magazine and Journal.

53 Harley Street,
May 23, 1865.

Gentlemen,

I HAVE received the following communication from Mr. Stone, of the Observatory at Greenwich, in reply to some questions put by me to the Astronomer Royal on the subject of Mr. Croll's valuable paper "On the Physical Cause of the Change of Climate during Geological Epochs," which appeared in the August Number of your Magazine for 1864. Feeling sure that it will be of use to Mr. Croll and those geologists and astronomers who may be following up the important line of investiga-

* Liebig's *Annalen*, October 1864.

tion lately opened up by him, I have asked Mr. Stone to allow his calculations to be printed, without waiting till others of a more complicated kind, and bearing on the same subject, are completed.

I have the honour to be, Gentlemen,
Your obedient Servant,
CHARLES LYELL.

To Sir Charles Lyell, Bart., F.R.S., &c.

Royal Observatory Greenwich,
SIR, May 12, 1865.

The Astronomer Royal has placed in my hands your letters of 1865, March 6, March 20, and May 8, together with Mr. Croll's papers, which I return: in part reply to your letters I have the honour to forward the following information.

Assuming the sun's mean horizontal equatorial parallax $=8''\cdot943$, and the Astronomer Royal's value of the equatorial semidiameter of the earth $= 3962\cdot822$ miles, I find the mean distance of the sun from the earth $= 91{,}400{,}000$ miles. From Leverrier's paper in the *Connaissance des Temps*, 1843, I extract the following values of the excentricity of the earth's orbit:—

For the year 1800 . . .	$=0\cdot0168$
Maximum value	$=0\cdot0778$

From computation, I find for the values of the excentricity 210,065 years ago the value

$$0\cdot0575.$$

These data give me the following results:—

Years ago.	Greatest distance of ⊕ from ⊙	Least distance of ⊕ from ⊙.	Difference.
65	92935521	89864479	3071042
210065	96655504	86144496	10511008
Maximum excentricity.	98506355	84293645	14212710

I consider that as the differences given above are nearly as 3 : 11 : 14, whatever climatic changes may have taken place through the existence of the absolute maximum excentricity, corresponding and very slightly inferior changes must have taken place about 210,065 years ago. As this period may therefore turn out to be geologically interesting, I forward the values of the excentricity and longitude of the perihelion of the earth's orbit about this period. The longitudes are referred to the mean

line of equinoxes for the year 1800, and are given merely to show the degree of rapidity of change of its position.

TABLE.

Years ago.	Value of *e*.	Longitude of ⊕ perihelion.
170,065	0·0437	228° 7′
180,065	0·0476	209 22
190,065	0·0532	190 4
200,065	0·0569	168 18
210,065	0·0575	144 55
220,065	0·0497	124 33
230,065	0·0477	102 49

I may mention that the above represents the maximum state of the excentricity during the last 500,000 years.

With respect to the precession of the equinoxes, although it would not be correct to assume the uniformity of motion during the immense periods of time here under consideration (for the accumulative error would become large), it will certainly lead to no sensible error to assume its values *at* the time 210,065 years ago as not very different from the present one.

It will be seen from the Table, that the change of excentricity and longitudes of perihelion are exceedingly slow as compared with the motion of the line of equinoxes; that if Mr. Croll's theory is correct, we must have had, when the excentricity is larger, alternate changes of climate from one of extreme cold to one of great equability.

The main point of the argument, as I understand it, of change of climate depending upon the excentricity of the earth is as follows. It is true that the amount of heat received varies but very slightly with changes in the excentricity; but the mean temperatures will depend as much upon the heat radiated off as upon that received; the amount of heat radiated off will certainly be less the shorter the time the temperature is below the mean, and the less it sinks below the mean; hence, *cæteris paribus*, a warm winter and a short one must raise the mean temperature, and a cold winter and a long one must lower this mean (the mean temperature). The effects produced, however, are terribly involved.

I have the honour to be, Sir,

Your obedient Servant,

E. J. STONE.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 486.]

May 4, 1865.—Major General Sabine President, in the Chair.

THE following communication was read:—

"On the Properties of Liquefied Hydrochloric Acid Gas." By George Gore, Esq.

In a former communication to the Royal Society "On the Properties of Liquefied Carbonic Acid," printed in the Philosophical Transactions for 1861 (also in the Journal of the Chemical Society, vol. xv., page 163)*, I described a mode of manipulation whereby various solid substances were introduced into that liquefied gas whilst under very great pressures (varying from 500 to 1100 pounds per square inch), and the action of the liquid upon them observed.

The experiments described in the present paper were made in a similar manner, but with some improvements in safety of manipulation, and in the mode of discharging the tubes, so as to recover the immersed solids in a satisfactory state.

The glass tubes in which the gas was condensed were about $\frac{3}{16}$ths of an inch internal diameter, and fully $\frac{5}{8}$ths of an inch external diameter. Each tube was, before bending, $11\frac{1}{4}$ inches long; it was bent, at $1\frac{1}{4}$ inch and $6\frac{1}{4}$ inches respectively from one end, to the form already described in the paper referred to, thus giving 5 inches in length for the salt, 5 inches for the acid, and $1\frac{1}{4}$ inch for the liquefied gas. These distances are essential; for if the quantities of acid and salt are not properly proportioned to each other, and to the remaining space in the tube, the liquefied product will be very small. The curve in the tube between the acid and the salt should be very gradual, and the other bend much less so. The end of the tube containing the salt should be constructed open, with a flange, and be closed securely by a plug of gutta percha in the same manner as the upper end.

The materials used were strong sulphuric acid and fragments of sal-ammoniac. Each tube was placed in a deal frame or box 10 inches high, 8 inches wide, and 4 inches from front to back, open at the back, and with a front or door of wire gauze. The tube was supported by a cork fitting into a hole in the side of the frame, and was secured within a notch in the cork by a ligature of wire. By means of this arrangement the acid and salt were brought into mutual contact by turning the box itself, without incurring the danger of putting one's hand inside the box and turning the tube alone, as in the former experiments.

The annexed figures (1 & 2) represent the position of the box, 1st, when charged and ready for the decomposition of the sal-ammoniac; and 2nd, after the decomposition is completed. The arrows indicate the direction in which the box is turned.

The action at first should be very slow; otherwise the bubbles

* The reader is referred to the above communication for details of information respecting the apparatus employed and manipulation adopted.

of gas will convey the sulphuric acid into the short end of the tube, and endanger the purity of the liquefied hydrochloric acid. The action of the acid was less violent than when generating carbonic acid, and the process was less frequently stopped by clogging of the tube. The liquefied gas was condensed in contact with the various solid bodies by application (from behind) of cotton wool, wetted with ether, to the short end of the tube, as in the former experiments.

Each tube was discharged of its contents by taking hold of it with an ordinary wooden screw clamp support, and immersing its lower end in a vessel of nearly boiling water behind a protecting screen. The explosion quickly occurred, generally without fracture of the tube, and the substances operated upon could in nearly all cases be readily extracted for examination without suffering injury by coming into contact with the saline contents of the tube. Powdered substances, however, were frequently lost during the discharge, owing to the sudden expansion of the gas in their pores expelling them from the small glass cup. The great degree of pressure (probably about 700 pounds per square inch and upwards) to which the various substances were subjected, frequently made them very hard.

Fig. 1.

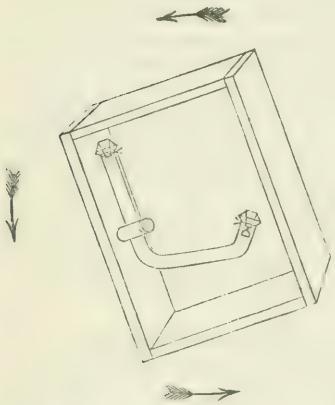
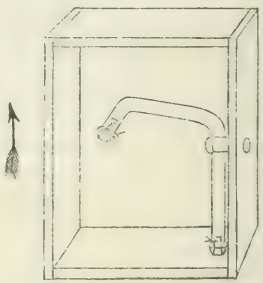


Fig. 2.



The chief inconvenience met with in these experiments arose from the action of the liquefied acid upon the upper gutta-percha stopper, causing the acid to become dark red-brown and opaque, and preventing accurate observation of the substances—also, on discharge of the tube, causing the glass cup and its contents to become coated with a tenacious film of gutta percha. To obviate this inconvenience as much as possible, the inner end of the upper stopper was carefully coated with melted paraffin.

During the early part of each experiment, the liquefied acid was repeatedly poured back, and redistilled by the application of ether, in order to free it from colour imparted to it by the stopper, and also to make its solvent or other action upon the immersed body more rapid. The action of the liquid acid upon the bodies was only

continued a few days; and in many cases the acid was not in a liquid state the whole of the time, but only at intervals; in all cases, however, the period of immersion was abundantly sufficient for the liquefied acid to produce its full effect.

The effects in nearly all cases were of so distinct a character, and the conditions under which they were produced so definite, as not to require repetitions of the experiments; but those which were in any respect uncertain were repeated, and those also which were of an important or striking character were likewise repeated, in order to remove the least shadow of a doubt that might be raised respecting them.

The liquid acid is a very feeble conductor of electricity. Two fine platinum wires, immersed in it $\frac{3}{8}$ths of an inch in length and $\frac{1}{10}$th of an inch asunder, and connected with a series of 10 Smee's elements, evolved no perceptible bubbles of gas, and produced only a moderate deflection (amounting to 23 degrees) of the needles of a sensitive galvanometer; and this amount of conduction might possibly have been due to a minute trace of oil of vitriol mixed with the liquid acid. In a second similar experiment, with the wires $\frac{1}{16}$th of an inch apart, not the slightest conduction occurred on using the same battery-power, but by employing the secondary current of a strong induction-coil with condenser attached, conduction and a steady deflection of the needles of the galvanometer (26 degrees) took place, gas being freely evolved from the negative wire only. On separating the brass points of the secondary terminals beyond the distance of the thickness of a thin address card, sparks ceased to pass between those points, and gas was evolved copiously in the liquid acid, apparently in the *mass* of the acid between the two platinum wires as well as at the wires themselves; two similar platinum wires in dilute hydrochloric acid in the same circuit evolved very little gas. It is probable that much of the gas evolved in the liquefied acid was not a product of electrolysis, but simply the acid itself volatilized by the thermic or other action of the current. No sparks occurred at any time in the liquid acid. It is evident therefore that liquefied hydrochloric acid gas is a very bad conductor of electricity, but it is not nearly so powerful an insulator as liquefied carbonic acid gas.

The following experiments illustrate its chemical, solvent, or other action upon various substances immersed in it. The quantity of the solid substances employed was in nearly all cases very small in proportion to that of the liquid acid in contact with them, and in many cases did not amount to one-twentieth of its volume.

A piece of charcoal remained unchanged at the end of ten days, the acid being in a liquid state in contact with it at intervals. A fragment of fused boracic acid did not lessen in bulk or alter in appearance in seven days. White phosphorus was undissolved and unchanged in nine days, and remained equally inflammable. A fragment of ordinary sulphur did not dissolve or alter in several days. Fragments of vitreous black selenium did not dissolve or change in six days. Iodine dissolved rather freely, and quickly

formed a purple-red solution. A piece of pentachloride of phosphorus softened in the gaseous acid, and dissolved quickly and completely in the liquid acid, forming a colourless solution. A fragment of sesquicarbonate of ammonia swelled and became full of fissures in the gaseous acid, but neither evolved gas nor dissolved when the liquid acid came into contact with it; after three days' intermittent immersion in the liquid acid, the saline residue evolved no gas on immersion in dilute hydrochloric acid. A piece of sal-ammoniac, immersed almost constantly during nine days, remained undissolved and unchanged.

Potassium evolved no gas when the liquid acid came into contact with it; after eight days it was sometimes enlarged in bulk, and from the outset it was of a white colour; it did not at all dissolve. In a second experiment the results were precisely similar; after three days' intermittent immersion the saline residue showed no signs of containing free potassium on immersing it in dilute hydrochloric acid. Anhydrous carbonate of potash in powder evolved no gas on first coming into contact with the liquid acid; after three days' occasional immersion it remained undissolved, and the residue evolved no carbonic acid on immersion in dilute hydrochloric acid. A crystal of chloride of potassium did not dissolve or change in appearance by four hours' immersion in the liquefied acid. Powdered chlorate of potash imparted a yellow colour to the liquid acid, and did not lessen in bulk during three days' constant immersion; the upper gutta-percha stopper became quite white at its inner end. A crystal of nitrate of potash became of a brownish colour before the gas liquefied, and remained undissolved after six days' intermittent immersion; the upper gutta-percha stopper was unusully acted upon, and of a nankeen colour.

Sodium became white and swelled largely before the gas liquefied. No visible gas was evolved by it in the liquid acid. After three days' intermittent immersion the residue contained no sodium in the metallic state, and no portion of it imparted a blue colour to damp litmus paper. Anhydrous carbonate of soda in powder immersed one hour and a quarter in the liquid acid evolved no visible bubbles of gas, and lost its alkaline reaction (with litmus paper) to about three-fourths of its depth. A fragment of fused sulphide of sodium produced a slight sublimate of a yellowish-white colour in the gaseous acid, and turned of a yellowish-white colour. It evolved no visible gas in the liquefied acid*. After three days' variable immersion it was of a yellowish-white colour, and somewhat enlarged in bulk; the residue evolved no sulphuretted hydrogen by immersion in dilute hydrochloric acid, and its solution gave a perfectly white precipitate with acetate of lead, and imparted no dark colour to sulphate of copper.

Precipitated carbonate of baryta in powder evolved no visible gas

* Probably the sulphuretted hydrogen set free was in a liquid state, and therefore no bubbles of gas appeared. I found by experiment that hydrochloric acid and hydrosulphuric acid, generated together and condensed into a liquid state, did not form two separate strata of liquid.

by immersion in the liquid acid; it remained undissolved and unchanged in appearance during three days' immersion; the residue evolved a minute quantity of gas by contact with dilute hydrochloric acid. Precipitated carbonate of strontia in powder behaved like carbonate of baryta; the residue, after three days' immersion, was lost during the discharge. A minute fragment of anhydrous Bristol lime exhibited no solution or alteration by nearly constant immersion during eight days in the liquid acid. On removal from the tube, it imparted a strong blue colour to neutral litmus paper by slight friction. On fracture it was found similarly alkaline throughout, and exhibited a slight change of colour, extending from its surface to the centre, as if the gas or liquid had been forced into its pores. In a second experiment of three days' intermittent immersion, precisely similar effects were obtained. Several minute fragments of very soft marble were immersed in the liquid acid at intervals during seven days. No gas was evolved when the liquid touched them. On removal from the acid, their physical characters appeared unaltered; they were insoluble in water, but quickly dissolved in dilute hydrochloric acid, with copious evolution of gas. A fragment of bone-earth did not dissolve or alter in appearance during seven days.

Bright magnesium ribbon slowly became dull in the liquid acid, without visible evolution of gas; after seven days' intermittent immersion it was still (with the exception of a thin film) in the metallic state. In a second experiment of three days' constant immersion, similar effects occurred; the residue dissolved and floated in dilute sulphuric acid, with copious evolution of gas. A wire of magnesium and one of platinum immersed in the liquid acid, and connected with a sensitive galvanometer, evolved no perceptible electric current, and only a barely perceptible current after two days of constant immersion. Calcined magnesia in powder did not dissolve or alter in appearance during four days' nearly constant immersion. Oxide of cerium (containing some oxide of didymium and lanthanum) remained undissolved and unchanged in colour during nine days; the residue was insoluble in water. Metallic aluminium became dull in the gas, and quickly dissolved, with evolution of gas, when the liquid acid came into contact with it, and formed a colourless solution. A wire of aluminium and one of platinum, immersed $\frac{1}{9}$th of an inch apart in the liquefied acid, and connected with a sensitive galvanometer, produced a steady deflection of $12\frac{1}{2}$ degrees, the aluminium being positive; the deflection gradually increased to 17 degrees in one hour, and two layers of liquid formed, the lower one brown in colour, and the upper one nearly colourless. The conductivity of the liquid acid was probably increased by the metallic aluminium dissolved in it. Precipitated alumina did not visibly alter or dissolve during six days; the residue deliquesced in damp air. Precipitated silica in powder did not dissolve or visibly alter during four days. Precipitated titanic acid in powder (pale flesh-colour) slightly dissolved in seven days.

A fragment of fused tungstate of soda did not alter in bulk during

ten days; it had then acquired a superficial green colour. Molybdic acid in powder turned dark green, but remained undissolved at the end of nine days. Native sulphide of molybdenum remained undissolved and apparently unchanged during two days. Molybdate of ammonia in powder became yellowish green in the gas; it became grass-green in colour in the liquefied acid, but did not dissolve in four days. Sesquioxide of chromium in powder did not dissolve in six days, but became of a dull blackish-brown colour. A fragment of anhydrous yellow chromate of potash became red before the gas liquefied, but did not dissolve or otherwise alter in the liquid acid. Sesquioxide of uranium became of a paler yellow colour in the gas, but did not dissolve in the liquid acid in six days; the residue was entirely soluble in water. Precipitated black oxide of manganese in powder, and free from water, became quite white in the gas; it remained white in the liquid acid without evolving visible bubbles of gas, and did not lessen in bulk in seven days. A crystal of permanganate of potash softened and swelled in the liquid acid, but did not dissolve in five days; it remained of a dark colour; the residue placed in distilled water produced no coloration.

A crystal of metallic arsenic remained perfectly bright and unchanged in bulk during three days' immersion. Arsenious acid in powder quickly liquefied in the gas, and dissolved to a colourless solution in the liquid acid. A crystal of arsenic acid softened before the gas liquefied, and dissolved quickly and freely in the liquid acid to a colourless solution. Bisulphide of arsenic in powder did not dissolve in six days, but became slightly less red and more yellow; a slight yellowish-white sublimate occurred in the tube during the generation of the gas. Teriodide of arsenic in powder slightly dissolved to a purple-red liquid; apparently only a trace of its iodine was extracted, as its bulk was not visibly less in three days. A crystal of bright antimony remained perfectly bright and unchanged after nine days' intermittent immersion. Precipitated teroxide of antimony became partly liquid before the gas liquefied; it dissolved in the liquid acid quickly and rather freely, and made a colourless solution. A fragment of precipitated antimonic acid did not dissolve in six days. A fragment of black tersulphide of antimony evolved a film of yellowish-white sublimate, and lessened in bulk before the gas liquefied; it decomposed and dissolved in the liquid acid in about a quarter of an hour, and formed a colourless solution which exhibited no further change during seven days. A fragment of bright metallic bismuth remained undissolved and unchanged in the liquid during three days.

Bright zinc evolved no visible gas in the liquid acid, and was not perceptibly corroded in three days. Oxide of zinc slowly dissolved in seven days. Metallic cadmium evolved no gas in the liquid and was not sensibly corroded in three days. Precipitated carbonate of cadmium evolved no visible gas in the liquid acid, and remained undissolved and unchanged in appearance during seven days. Yellow sulphide of cadmium evolved a trace of white sublimate before the gas liquefied; in the liquid acid it became quite

white, and remained undissolved in seven days; on removal it was hard in texture and quite white throughout, and evolved no odour of sulphuretted hydrogen or separation of sulphur on treatment with strong nitric acid. Bright tin evolved no visible gas in the liquid acid; after ten days' intermittent immersion it was converted, to some depth of its substance, into a bulky white solid with deep fissures. In a second experiment of three days' immersion, similar results occurred; all the tin was corroded except a minute fibre in the centre, the white solid was imperfectly soluble in water, but instantly soluble in dilute hydrochloric acid. Binoxide of tin in powder did not dissolve in seven days; the residue was white and insoluble in water. A crystal of protochloride of tin softened before the gas liquefied, and partly dissolved in the liquid acid in four days. Bright metallic thallium evolved no gas in the liquid acid, and was only superficially blackened without further corrosion after three days' immersion. Metallic lead did not evolve visible gas in the liquefied acid; it became blackened at first, and in ten days was corroded deeply to a white substance. Red oxide of lead quickly became white in the liquid acid, but did not dissolve in seven days; it was then quite hard, white throughout, and not readily soluble in water. Precipitated carbonate of lead evolved no visible gas in the liquid acid, and remained undissolved after three days' immersion; the residue evolved no gas by contact with dilute hydrochloric acid. Precipitated sulphide of lead in powder produced a faint film of white sublimate in the gas, and by a few hours' immersion in the liquid acid became wholly white; it did not dissolve during seven days, and was then quite white throughout, and not readily soluble in water. Yellow iodide of lead did not dissolve in seven days, but became of a purplish brick-brown colour and evolved a strong odour of free iodine; it produced yellowish-brown stains upon paper. Yellow chromate of lead evolved at first (in the gaseous acid) a small quantity of deep-red vapour, which condensed as a red moisture near it on the tube; the chromate became white in the gas, and did not dissolve in the liquid acid in three days; it was then a soft white solid, not freely soluble in water, and imparted a faint greenish tint to water.

A minute fragment of iron remained bright, and evolved no gas when the liquid acid came into contact with it; after nine days of intermittent immersion it was only slightly tarnished, and on removal from the acid was found otherwise unaltered. A fragment of fused sulphide of iron produced a faint film of whitish sublimate at first, but evolved no bubbles of gas on contact with the liquid acid; it did not dissolve or alter in appearance. A second fragment constantly immersed during three days behaved similarly; it was as hard as before immersion, and evolved sulphuretted hydrogen freely in hot dilute sulphuric acid. A crystal of green vitriol became yellowish white and opaque in the liquid acid, but did not diminish in volume in six days; the residue was a soft opaque yellowish-white solid. Oxide of cobalt in powder exhibited no change or solution during three days; on removal it was found to be very hard, of a light-

brown colour, and dissolved in water, producing a pink solution with separation of black oxide. Peach-coloured carbonate of cobalt evolved no visible gas in the liquid acid; it became greenish blue, but did not lessen in bulk in three days; the residue became pink in the air, and dissolved almost completely in water, forming a pink liquid; it also dissolved in dilute hydrochloric acid without evolving bubbles of gas. Anhydrous chloride of nickel did not dissolve in the liquid acid in six days. Metallic copper soon lost its brightness in the gas; it evolved no gas in the liquid acid, and was only slightly corroded after seven days. Black oxide of copper became of a lighter colour in the liquid acid, but did not lessen in bulk in seven days; the residue was a greenish and yellowish white powder, which instantly turned black in water, forming a pale-blue solution, and left black oxide of copper. A crystal of blue vitriol became of a light brown colour in the liquid acid, but did not dissolve in six days; on removal it was found to be a brown soft solid. Protoxide of mercury became white in the gas, and did not dissolve by constant immersion in the liquid acid in four days; the residue was a white solid, soluble in water. Vermilion in powder slowly changed in the liquid acid in three days to a pinkish-white solid, but did not dissolve. Scarlet iodide of mercury in powder imparted a red colour to the liquid acid, but did not lessen in bulk or change in colour during three days; the residue lost its red colour on the application of heat. A fragment of protochloride of mercury did not visibly alter in the liquid acid in four days. Metallic silver did not dissolve or become much corroded during seven days. Oxide of silver became white in the liquid acid in one day, but did not dissolve. Precipitated chloride of silver in powder did not visibly alter or dissolve during sixteen days. Metallic platinum was unaffected in the liquid acid.

Oxalic acid was slightly dissolved in the liquid acid in three days without change of colour. Uric acid remained undissolved and unchanged during three days. Paraffin did not appear to be dissolved or affected in nine days. Gutta percha was quickly acted upon; it imparted to the liquid acid, first a red, and ultimately a dark-brown colour; it appeared also to dissolve in the acid to some extent, and on discharging the tubes was left behind as a tenacious coating upon the adjacent parts. Gun-cotton was unaffected in the liquid acid. Cotton was not visibly altered in two days. Solid extract of litmus dissolved slightly, forming a faintly purple blue or inky solution; it became of a dark red colour and enlarged in bulk; the residue formed a perfect solution in water; the solution was red in colour.

Remarks.—The foregoing experiments show that liquid hydrochloric acid has but a feeble solvent power for solid bodies in general. Out of 86 solids it dissolved only 12, and some of those only in a minute degree; of 5 metalloids it dissolved 1, viz. iodine; of 15 metals it dissolved only 1, viz. aluminium; of 22 oxides it dissolved 5, viz. titanic acid, arsenious acid, arsenic acid, teroxide of antimony, and oxide of zinc; of 9 carbonates it dissolved none; of 8 sulphides it dissolved 1, viz. tersulphide of antimony; of 7 chlorides it dis-

solved 2, viz. pentachloride of phosphorus and protochloride of tin; and of 7 organic bodies it dissolved 2.

The results show also that liquid hydrochloric acid in the anhydrous state manifests much less chemical action upon solid bodies than the same acid when mixed with water as under ordinary circumstances; for instance, the difference of its action upon magnesium, zinc, cadmium, and even aluminium, under the two conditions, is very conspicuous. This may arise in a great measure from its feeble solvent capacity—insoluble films forming upon the surface of the bodies immersed in it preventing its continued contact and further action. This want of contact could hardly have been the case in the remarkable instance of caustic lime: here was a powerful and true acid (*i. e.* a hydrogen acid) and a powerful base; each in a nearly pure state; both possessing under ordinary circumstances a very powerful chemical affinity for each other; the one a liquid, and the other a porous solid; brought into intimate contact by an enormous pressure forcing the liquid into the porous solid; the solid base being very small in bulk, and the liquid acid largely in excess, probably fifty times the quantity necessary for its saturation; and the action extended over a far greater period of time than would in the presence of water been at all necessary: nevertheless no perceptible chemical action occurred; the two remained totally uncombined.

It must not be overlooked that the results are partly due to anhydrous hydrochloric acid in the *liquid* state, and partly to the same acid in the *gaseous* state, under great pressure, the one class of effects not being eliminated from the other in the present experiments; it is probable that if the substances could have been submitted to the action of the *liquid* acid *alone*, the chemical effects would have been much smaller even than they were. For instance, the action upon potassium, sodium, and tin appeared to be due to the influence of the acid in the gaseous state, as no gas was perceptibly evolved by these metals in the liquid acid. In the cases of potassium and sodium (the latter in particular) it is perhaps possible, though highly improbable, that the whole of the metal had been corroded before the liquid acid touched it; but with tin this was certainly not the case, some metallic tin being left uncorroded at the end of the experiment.

Oxides in general, with the exception of lime and certain others which do not readily combine with aqueous hydrochloric acid, were slowly converted in a greater or less degree into chlorides. Carbonates also, except that of lime, were in general converted in a greater or less degree into chlorides.

Such carbonates as were decomposed evolved no visible bubbles of gas in the liquid acid: this may be explained on the supposition that they were previously completely decomposed by the *gaseous* acid during the process of generation (this, however, was not the case with carbonate of soda), or that the liberated carbonic acid was in the *liquid* state and was *dissolved* by the liquid hydrochloric acid. In my former paper it was shown that liquid carbonic and hydrochloric

acids generated and condensed together did not form two separate strata of liquid.

Sulphides were in some cases converted into chlorides; in other cases not so; in nearly all cases a trace of whitish sublimate was produced in the *gaseous* acid. The chlorate and nitrate of potash were both decomposed.

I may here take the opportunity of stating that tubes charged with liquid carbonic acid in October 1860 suffered no leakage by February 1865.

GEOLOGICAL SOCIETY.

[Continued from p. 405.]

April 26, 1865.—W. J. Hamilton, Esq., President, in the Chair.

The following communications were read:—

1. "On the Character of the Cephalopodous Fauna of the South Indian Cretaceous Rocks." By Dr. F. Stoliczka.

In this paper the author gave a summary of the more important facts brought to light by the examination of the Cretaceous Cephalopoda of Southern India, which was begun by Mr. H. F. Blanford, and continued by himself, giving, first of all, a brief notice of what had been done previously by other observers, and a sketch of Mr. Blanford's subdivision of the strata into the Ootatoor (or Lower), the Trichinopoly (or Middle), and the Arrialoor (or Upper) groups. All the genera characteristic of European Cretaceous faunæ were stated to be well represented, the whole assemblage having a Middle Cretaceous aspect. The number of species of the different genera occurring in each of the three subdivisions was then given, as also the distribution of the groups of the genus *Ammonites*—the most striking and abnormal feature being the intimate association of three species of that genus, belonging to the Triassic group "Globosi," with true Cretaceous fossils. Dr. Stoliczka then discussed the relation of this Indian fauna to those of the European Cretaceous rocks, and illustrated his remarks by a table showing the geological range in India and in Europe of the species that are common to both areas. He came to the conclusion that for the present the lowest of Mr. Blanford's subdivisions (the Ootatoor group) may be considered to be of the age of the European Gault; while the uppermost (the Arrialoor group) does not seem to correspond to a higher division than D'Orbigny's Sénonien.

2. "On the Growth of Flos Ferri, or Coralloidal Arragonite." By W. Wallace, Esq.

The author first described the physical features of the Meldon Mountains, in Westmoreland, and endeavoured to show that they bore certain relations to the geological structure of the country, and that the number and size of the joints varied with the elevation of the rocks, and their position in relation to the valleys. After the formation of the joints, the minerals occurring in the veins in their neighbourhood were stated to be acted upon by decomposing-agents, and it was therefore inferred that the amount of decomposition in

veins and in rocks is proportional to the amount of their elevation above the sea.

Mr. Wallace then stated that Arragonite is produced only after the strata are traversed by joints, and that the branched Arragonite very rarely occurs, being found only in caverns and old workings. Two of these caverns have come under his notice, and were described in detail; one of them is in the north vein of the Silver Band Mine, and the other near one of the principal veins of the Dufton Fell Mine. Finally he discussed the causes and conditions necessary to the formation of this Coralloidal Arragonite, and came to the conclusion that the theory of a circulation, through the pores of the spar, of fluids holding its component parts in solution is the only one that harmonizes with the varied phenomena observed in the two caverns he had described.

3. "Notes on presenting some rhomboidal specimens of Ironstone, &c." By Sir J. F. W. Herschel, Bart., K.C.H., F.R.S., F.G.S., &c. With a Note by Captain T. Longworth Dames.

Most of these specimens came from a quarry at Clanmullen, near Edenderry, King's County, and the remainder from the Collingwood Quarry, in the Weald of Kent. The Irish specimens are siliceous, containing some oxide of iron and a little manganese, and are homogeneous throughout. They all agree in the sharpness of definition and the exact parallelism and evenness of the flat surfaces; but, like those from the Weald, they are not constant in form or size, and sometimes are very irregular in angle and in the parallelism of opposite sides. The Wealden specimens, however, are all closed boxes, each containing a rhomboid of hardened sandstone, the outer case being highly ferruginous—in fact, the "Ironstone of the Weald." Sir John Herschel endeavoured to account for the formation of the boxes, and Captain Dames added a Note stating the circumstances under which the Irish specimens occur.

LXXV. *Intelligence and Miscellaneous Articles.*

ELECTRICAL MACHINE WITH A PLATE OF SULPHUR.

BY M. RICHER.

IT is known that M. Ste.-Claire Deville has found that if sulphur is frequently remelted and suddenly cooled it is changed into red sulphur. I have further noticed that in melting sulphur which has thus crystallized several times, in special circumstances of cooling, it assumes a kind of temper; and this molecular condition appears to be permanent. I have been able to obtain plates or disks of sulphur 2 to 3 centimetres in thickness and a metre in diameter. They have a certain amount of toughness, but are somewhat more fragile than glass; but as they are very little hygroscopic, and can be obtained at a very low price, they may be usefully employed in constructing frictional electrical machines. Several of these made more than a year ago work regularly.—*Comptes Rendus*, January 30, 1865.

CHEMICAL AND MINERALOGICAL CHARACTERS OF THE METEORITE OF ORGUEIL.

Daubrée, Cloëz, Pisani, and Des Cloizeaux have communicated to the French Academy papers on the physical and chemical characters of the Orgueil meteorite. In outward appearance it resembles an earthy lignite. The dark mass contains minute grains of a bronze-yellow substance having a metallic lustre and a high density, which permits its easy separation from the main portion of the meteorite by levigation: observed under the microscope, these particles are seen to be distinct hexagonal tables; they are strongly attracted by the magnet, and have all the physical and chemical properties of magnetic pyrites. A very marked characteristic of the meteorite is, that when placed in water it falls to powder, and a portion of it is in such an extreme state of mechanical division that it remains a long time in suspension in the water, and passes through the closest filter-paper. The density of the meteorite, taken in pure benzine, gave Cloëz 2·567. An analysis by this chemist shows it to contain a considerable amount of magnetic oxide of iron, besides silicates, protosulphide of iron, traces of nickel and chromium, 5·92 per cent. of carbon (probably of the graphitic form), 9·06 per cent. of water, and 5·30 per cent of matter soluble in water, consisting of chlorides of ammonium, sodium and potassium, and sulphates of magnesia, soda, &c. It is decomposed by hydrochloric acid with evolution of sulphuretted hydrogen, giving a greenish yellow solution and leaving a black residue amounting to 7·6 per cent.; this residue when heated with excess of air burns, and leaves a grey substance amounting to 2·2 per cent.

Pisani confirms the observations of Cloëz as to the peculiar comportment of this stone when treated with water, and calls attention to its porosity, and to the fact that this accounts for the facility with which the sulphides have become oxidized to sulphates and hyposulphites, and for the avidity which a dried specimen of it has for water. An experiment showed that a specimen dried at 110° C. absorbed 7 per cent. of water in a few hours when simply exposed to the air. Pisani found 3·35 per cent. of matters soluble in water in operating on about 18 grammes of the undried substance. This contained hyposulphurous acid 0·48, sulphuric acid 1·40, chlorine 0·08, magnesia 0·30, lime 0·16, potash 0·60, soda, ammonia &c., and loss 0·77. Alcohol took up 0·37 per cent. which proved to consist chiefly of sulphur. Pisani's tabulated results give for the composition of the whole meteorite:

SiO^3.	MgO.	FeO.	MnO.	CaO.	NaO.	KO.	Al^2O^3.
26·08	17·00	6·96	0·36	1·85	2·26	0·19	0·90,

together with chromic iron 0·49, magnetic iron 12·03, nickeliferous sulphide of iron 16·97, water and supposed organic substances 14·91=100. This gives for the oxygen ratio of the oxides and silica 9·98 : 13·90=3 : 4.

Pisani proved the presence of magnetic iron by dissolving the mineral in hot nitric acid, which decomposed the silicate and the sulphides, and left a black magnetic residue. The nickel was proved to

exist as sulphide by treating the substance with sulphide of ammonium, which dissolved out sulphide of nickel. The stone contains 55·60 per cent. of silicates; and taking into account the water determined by Cloëz, the oxygen ratio of the silicate is that of serpentine. If the alumina is due to anorthite, it will give 2·42 per cent. of this felspar contained in the meteorite. Subsequent observations by Des Cloizeaux, Pisani, Daubrée, and Cloëz prove that this remarkable meteorite contains minute rhombohedral crystals of a double carbonate of magnesia and iron, and Cloëz obtained a little more than a half per cent. of carbonic acid from a portion of the meteorite operated upon.—Silliman's *American Journal* for March 1865.

NOTE ON THE PROPAGATION OF ELECTRICITY THROUGH METALLIC VAPOURS PRODUCED BY THE VOLTAIC ARC. BY A. DE LA RIVE.

In following out my investigations upon the propagation of electricity in greatly rarefied elastic fluids, I have been led to study this propagation in the vapours of various metals.

The apparatus that I make use of for this purpose consists of a large glass balloon, furnished with four tubulures and supported on a foot. The two tubulures at the extremities of the horizontal diameter are furnished with leather plugs traversed by metal rods, to which are fitted the metallic or charcoal points, which serve to produce *voltaic arcs* by means of a Bunsen's battery of 60 to 80 pairs. The two tubulures situated at the extremities of the vertical diameter serve for the passage of two brass rods, terminated by metal balls, between which passes at the same time the *electric jet* of a Ruhmkorff's apparatus. The balloon, after a vacuum has been produced in it, is filled with thoroughly dried nitrogen, which is rarefied to a pressure of from 2 to 3 millims.; then the electric jet is passed through, its intensity being measured by means of the process of derivation described by me in a previous communication.

After the constancy of this intensity has been ascertained, the horizontal metallic points are brought near to each other, so as to produce the voltaic arc, which acts here solely as a source of heat. Care must be taken to maintain this arc for some minutes; then at a certain moment the intensity of the electric jet is seen to be considerably increased; at the same moment the colour of this jet, which was of a deep rose in the nitrogen, acquires quite a different tint, which varies according to the nature of the conducting points between which the voltaic arc passes. This new appearance lasts for some moments after the cessation of the arc; and it is even at this period that it is most remarkable, as it no longer has to suffer by contrast with the dazzling light of the arc.

The voltaic arc was successively produced between points of *silver, copper, aluminium, zinc, cadmium, and magnesium*, and between two points of *gas-coke*, all these materials being capable of acquiring a gaseous state in consequence of the high temperature which is produced. With points of silver and zinc the electric jet exhibits a very distinct blue colour, darker with zinc than with silver. With points of copper the tint is a very dark green; with cadmium apple-

green; with magnesium very bright green; and with aluminium whitish green. With coke-points the colour of the jet is bright blue, which becomes bluish when the arc ceases; this is due to the production of a little carburetted hydrogen gas, as I have ascertained directly.

It is in the upper part of the balloon, to which the metallic vapours produced by the voltaic arc ascend, that these effects are most distinct. The streaks or stratifications of the electric light are still more marked in these vapours than in rarefied gases.

The augmentation of the intensity of the jet is most considerable in the vapours of silver and copper. The galvanometer passes suddenly from 30° to 60° at the moment when the jet indicates, by its change of colour, that it is transmitted through the vapours of those metals. The increase, although less, is still very distinct in the vapour of aluminium. It is much less, only from 10° to 20°, with the vapours of zinc, cadmium, and magnesium. Lastly, the increase is very great with the vapour arising from the arc formed by two coke-points; but here the effect is more complex in consequence of the production of small quantities of carburetted hydrogen gas, which it is difficult to avoid.

I have also employed iron and platinum points. With the former I have certainly observed a change in the colour of the electric jet and a slight augmentation of its intensity; with the latter no effect was produced, except that the excessive elevation of the temperature caused an appreciable increase in the conductibility of the rarefied nitrogen, but too slight to be in any way compared with the augmentations of which I have spoken above. This last experiment proves that it is not to the heating of the rarefied nitrogen, but to the presence of the metallic vapour produced by the voltaic arc, that the very distinct augmentation of the conductibility above indicated is due. This does not imply that the temperature has no influence; its influence is, on the contrary, very sensible, and is manifested in all rarefied gases by a considerable increase in their conducting-power. I have already determined this increase for a great number of gaseous substances, and I shall make known the results that I have obtained in this respect when my work is more complete.

I will not conclude this notice without indicating *en passant* a phenomenon which I had the opportunity of observing in trying to produce the voltaic arc with points of various alloys, namely, that at these high temperatures all these alloys are decomposed. To observe this decomposition more satisfactorily, I take as the negative electrode a plate of coke; the point of alloy serves as the positive electrode, and consequently becomes heated and vaporized. Alloys of copper and zinc, copper and tin, copper and aluminium, platinum and silver, and of iron and antimony have all been decomposed at these high temperatures, and I have collected upon the plates of coke particles of the constituent metals of the alloys very distinctly separated. This is a case of dissociation by heat, which indeed might easily have been foreseen, and which comes as an addition to those curious examples indicated by M. H. Sainte-Claire Deville in his beautiful investigations.—*Comptes Rendus*, May 15, 1865, p. 1002.

APPLICATION OF THE ELECTRIC LIGHT (GEISSLER'S TUBES) FOR LIGHTING UNDER WATER. BY M. PAUL GERVAIS.

Of late years the electric light has been used for illumination under water. In the Channel and in the Mediterranean attempts were made by means of water-tight receivers of glass, in which works a regulator for bringing in contact carbons made incandescent by a battery, the elements of which are on board a vessel on which the trials are made. The part serving as lantern is sunk under water.

In some cases these attempts have been successful, and the light thus produced has been utilized either for submarine works or in fishing, which this method seemed to render more productive by attracting the fish. Yet the use of such apparatus is costly, and the manipulation difficult; moreover the light is in many cases too bright, and, besides, the entire arrangement is liable to numerous accidents, such as, for instance, the spilling of the liquid owing to the motions of the vessel.

There are, moreover, circumstances in which a less brilliant light is sufficient and even preferable. It would, therefore, be useful to construct an apparatus capable of working under water, and such that its total immersion would not stop its working. I thought that these results might be arrived at by means of *Geissler's tubes*, placing them in connexion with an exhausted receiver containing the elements of a battery and a coil for producing the electrical current by which these tubes are made luminous; and M. Ruhmkorff has constructed for me the following apparatus:—

Our receiver is a sort of bronze box mounted on four small feet, and the cover of which is made to fit hermetically by means of screws and of a vulcanized india-rubber washer. A ring on the cover serves to suspend the entire apparatus. The exhausted box contains two bichromate-of-potash elements closed in their turn by plates. The poles of the current furnished by the element can at pleasure be connected with the coil, and the induced current, by means of insulated wires, passes through the bottom of the apparatus to a Geissler's tube. This tube, of an appropriate form and filled with carbonic acid, is enclosed in a thick glass cylinder provided with copper armatures, and into which water cannot penetrate. This is the illuminating part of the apparatus.

With this instrument a soft but distinct light is obtained, resembling that which military engineers and miners now use. In some respects it resembles that which phosphorescent animals emit, though it is more intense. It can be seen at a considerable distance, even when the apparatus is worked under water at a depth of several metres. It cannot be doubted that it will attract fish as does the phosphorescence of certain species, and it might also be used to illuminate closed spaces situate below the surface of the water, or for making floating signals.

Captain Deroulx has seen this apparatus work in the port of Cette last September. In this experiment the apparatus was immersed for nine hours, and it illuminated during six hours under these conditions. The duration of its phosphorescence may be greater. A second trial at Port Vendres also succeeded.—*Comptes Rendus*, March 27.

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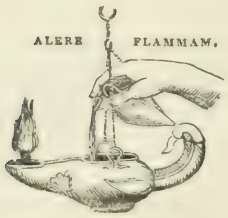
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